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BRAZILIAN EVIDENCE ON THE GENESIS OF THE DIAMOND.

THE extensive working of the "dry diggings" of South Africa has thrown a light on the original associates and probable mode of origin of the diamond, which it would be vain to look for in the ordinary type of diamond fields as known in other parts of the world, since all of these, with rare and imperfectly known exceptions, correspond almost exactly with the river washings of the Cape district. The group of mines about Kimberley have shown beyond a doubt that here at least the association of the diamond is with an eruptive rock of ultra basic composition, and, although opinions differ as to the exact mode of origin, all authorities seem to agree on the main fact that in some way this association is a genetic one. To one familiar with the Brazilian diamond fields this conclusion seems a startling one and utterly inapplicable to them. The evidence in its favor, if any exists, is either concealed and has been overlooked, or, as at first sight seems most probable, a totally different association is presented, necessitating the hypothesis of the formation of the mineral under a far wider range of conditions than has been admitted by the students of the Kimberley occurrence. In the present paper it is proposed to discuss the observations, in great part unpublished, that have thus far been made in Brazil bearing upon the question of the genesis of the diamond, with a view of seeing in how far they are in accord, or disaccord, with the much more complete observations in the South African mines.

With two exceptions, which will be more fully discussed below, all the known Brazilian diamond washings are in material — sand and gravel — which has clearly been transported from its place of origin and equally clearly contains the débris of a greater or less variety of rock types, some one or more of which may reasonably be presumed to have a genetic relation to the diamond. As the latter, however, is almost invariably found free in such deposits, or attached to the other elements by a cement, usually limonite, which is visibly of secondary origin, such deposits throw little light on the history of the gem. For the most part these deposits are of quite recent origin, having evidently been formed by the action of the present drainage agencies. In a few cases the gravel has been attributed[1] to the disintegration of conglomerates of various ages, which in one case is presumed to be very great. The age of the gem is thus carried back to a more or less remote geological period, but no other essential addition is made to its history. The concentrates of the rarer and heavier elements of these gravels obtained by the miners in their operations contain many rare and interesting minerals which have attracted the attention of mineralogists, but, thus far, the hopes that have been entertained of tracing the diamond to its original home by means of these satellites, have proved illusive, since none of them have proven to be sufficiently constant to give more than merely presumptive evidence. The few cases that have been reported of diamonds included in other minerals, as iron ores and rarely quartz and anatase, refer to minerals that are known to be readily formed by secondary action, and thus are not necessarily contemporaneous with their inclusions.[2]

The associates of the diamond in these gravels are naturally fragments of all the rocks capable of resisting decay and the

[1] DERBY: Am. Jour. of Sci., 1882, XXIV, p. 34.

[2] The specimen described by Eschwege, who attributed great importance to it (Geognostisches Gemälde, p. 430; Beiträge zur Gebirgskunde Brasiliens, pp. 213 and 341), and which is now in the Heuland collection in the British Museum, is apparently a cleverly executed fraud. The limonite and scorodite of the drusy cavity in which the diamond rests present the peculiar and very characteristic aspect of these minerals

wear of transportation that have contributed to the deposit, together with the isolated minerals derived from the breaking down of these rocks and of such others as have entirely disappeared as rock-masses and are now only represented by the more resistant of their constituents, which have been more or less completely assorted according to their resistance to disintegration and to wear, to their specific gravity and to the size of grain. These isolated minerals, the *formation* of the miners, who attach great importance to them, can in some cases, as of zircon, monazite, xenotime, etc., be referred with tolerable certainty to original eruptive rocks, though they may, and in many cases doubtless have, passed through others before reaching their present place in the gravels; others, as staurolite, disthene, etc., can with equal certainty be referred to metamorphosed clastics, but by far the greater part, as quartz, the iron and titanium oxides, tourmaline, garnet, and many others might be from either eruptives or metamorphosed clastics, or from both. The minerals which can with more or less probability be attributed to eruptive rocks, are not so predominant or so constant in their occurrence that any particular significance can be attached to them. Their evidence, so far as it goes, points rather to the acid eruptives, as granites, etc., than to the ultra basic types of the Kimberley district.

In only one Brazilian mine, so far as known, are basic eruptives a characteristic feature, and in this the conditions are such that the association seems to be accidental rather than genetic. This is the Agua Suja (dirty water) mine in the Bagagem district of western Minas Geraes, which has been excellently studied by Messrs. Gonzaga de Campos, Hussak, and Calogeras,[1] though

from the gold mine of Antonio Pereira, near Ouro Preto, which is the only known Brazilian locality of scorodite, but is not known as a diamond locality. The specimen is reported to come from the Abaieté district to the west of the São Francisco, but no other specimens of scorodite, or of limonite of this character, are known from that region, where, moreover, only gravel deposits had been worked, whereas the specimen in question is evidently from a mine, and not from a deposit of transported material.

[1] Gonzaga de Campos: Jazidas Diamantiferas de Agua Suja, Rio de Janeiro,

much is yet to be learned regarding this unique deposit. The region is characterized by inclined strata of micaceous schists, in part staurolitic, which are regarded as metamorphosed clastics, with intercalations of amphibolites, which are almost certainly metamorphosed eruptives. This schist series is cut by dikes of granite which, so far as observed, are characterized by muscovite either alone or in association with biotite, and which are generally tourmaliniferous. Quartz veins which frequently carry a little mica are also common. Upon this group of schists and granite rest horizontal beds of soft sandstones, with intercalated layers, or sills, of trap—augite-porphyrite or melaphyre—which are presumably of Triassic age. In the same region, although not definitely known in the immediate vicinity of the mine, there is another obscure eruptive group which has furnished material characterized by grains of pyroxine, perofskite, and magnetite, to beds of clay and impure limestone that overlie the sandstone and trap, and, in places, present something of the aspect of ash-beds or volcanic breccias. This group, though very imperfectly known, is certainly distinct from the traps, and its probable relations will be discussed below.

The diamond-bearing bed of Agua Suja is a thoroughly decomposed conglomerate, or breccia, in which both matrix and the included pebbles are transformed into clay. The original angular outline of the pebbles (or rather bowlders, as they are often of considerable size), can, however, be recognized, as also, in many cases, the type of rock to which they belonged. The various types of the schists and granites upon which the diamantiferous bed rests in part (in part also on sandstone and trap) are recognizable, as well as masses of the sedimentary and later eruptive series. Fragments of opal, which may be of secondary origin, constitute a peculiar feature when this mine is compared with others of the same region (Bagagem) or of the other diamantiferous regions of Brazil. Still more peculiar and

1891. E. HUSSAK: In the above cited pamphlet and in Relatorio da Commissão Exploradora do Planalto, Rio de Janeiro, 1894. J. P. CALOGERAS: Revue Universalle des Mines, XXIX, 1895.

characteristic is the presence in great abundance of magnetite and of a magnetite rock, which Dr. Hussak has succeeded in tracing to a special magnetite-perofskite type found by him near Catalão, in the state of Govaz.[1]

This last element of the diamantiferous bed cannot be referred to any of the known rocks of the region, but it points in the same direction as the above mentioned eruptive elements of the limestone and clay beds that are known to occur in the region and that may be presumed to extend to the immediate vicinity of Agua Suja. These elements, pyroxene, perofskite and magnetite, suggest a type of basic eruptive passing into an iron ore such as has actually been met with in the Jacupiranga district of the state of São Paulo in genetic relations with various nepheline-bearing rocks, the whole constituting a typical volcanic series.[2] As a somewhat similar volcanic center is known at Caldas[3] at no great distance from the Agua Suja region, there is a reasonable probability that another one may exist in the immediate vicinity, and that it may have furnished the problematic material of the diamantiferous bed. This last is not clearly referable to the present drainage conditions of the country and is very likely to prove to be an ancient conglomerate, or breccia, possibly in relation with the eruptive manifestation that is presumed to have contributed to its elements.

The Agua Suja occurrence thus offers a certain number of analogies with those of the Kimberley district which are entirely lacking in the other Brazilian localities, so far as they are known. It is especially to be noted that the absence of these analogies is as conspicuous at the nearest locality, Bagagem, only about twenty miles distant in the same river basin, as at any other. The country rock both at Kimberley and Agua Suja is horizontal, of approximately the same age (late palæozoic or early secondary) and with intercalated sills of trap of very similar character and composition, but which in both cases has no

[1] Neues Jahrbuch, 1894, II, p. 297.

[2] DERBY: Am. Jour. of Sci., XLI, 1891, p. 311.

[3] DERBY: Quart. Jour. of the Geol. Soc., 1887, p. 457.

apparent relation with the occurrence of the diamond; the deposit is conglomeratic, or brecciated, and in both cases the most characteristic elements of the conglomerate represent rocks of an ultra basic type, and in both the diamond is presumed to be an element of the cement rather than of the included pebbles; perofskite and garnet (pyrope) are characteristic accessories (garnets though frequent in most Brazilian sands and gravels are exceptionally rare in those of the diamond washings; those of Agua Suja present the rare cubic habit); the diamonds *seem* to be distributed with a considerable degree of uniformity throughout the mass.

With these analogies are, however, associated differences that are, in appearance at least, of equal if not greater importance. The Agua Suja deposit is a bed, not a volcanic neck; in it clastic elements, of a much more varied character than those of Kimberley greatly predominate over those of eruptive origin, which also are more varied in character; the cement is apparently clastic rather than eruptive; the eruptive elements, exclusive of the trap, probably represent basic phases of the nepheline- or augite-syenite type of rocks and not the peridotitic, and there is as yet no direct evidence that they have anything to do with the diamond; the original matrix of the garnets is unknown and there is no evidence that their association with the diamond and with the basic eruptives is direct and not accidental. If, as is quite within the range of possibilities, eruptive necks of the Kimberley type should be discovered in the Agua Suja region, or contemporaneous (?) sedimentary deposits of the Agua Suja type in the Kimberley district, some of these differences would doubtless become analogies, but that of the *probable* original rock type would still remain and would require an extension of the views at present held regarding the type of eruptive rocks with which the diamond is associated. If, as some hold in regard to the Kimberley occurrence, the diamond is the product of meta morphic action on carbon-bearing rocks and not an element of the eruptive rock itself, this last difference would lose much of its importance. In this case, the Kimberley and Agua Suja

occurrences would fall into line as phases of the same phenomenon of contact metamorphism, and to this it may be added that the, at present, striking differences between the latter and the other known Brazilian occurrences would be reconcilable.

Before leaving the topic of African analogies it may be mentioned that in another Brazilian diamond region, that of the river Abaiete, a porphyritic peridotite (picrite-porphyry) with perofskite, quite similar to that of Kimberley as described by Lewis and others, has been found. Its known occurrence is, however, at some distance from the diamond washings and no relation between the two would ever have been thought of if it had not been for the Kimberley occurrence.

In the oldest and best known of the Brazilian diamond fields, that of Diamantina in Minas Geraes, there is an apparent relation, first noted by Eschwege in 1822 and confirmed by all subsequent writers, between the distribution of the diamond and that of the quartzose rock known as itacolumite. Eschwege who first described this rock[1] recognized a schistose and a massive type, the latter often presenting a conglomeratic appearance and, occasionally, an apparent lack of conformability with the former.[2] As, however, both types, and the schists associated with the schistose one, were considered as constituting a single division of the primitive group and as having a special, and non-clastic, mode of origin, the two were not separated and the question of the conglomeratic character was left by Eschwege as an unsolved problem. The predominance of the massive type of itacolumite in the Diamantina region was noted and from this a genetic relation between the rock and the diamond was inferred, an hypothesis which has become deeply rooted in mineralogical literature. About 1840, and after the publication of Eschwege's various works, diamonds were actually discovered and worked in this rock at Grão Mogol, some 100 miles, more or less, to the northward of Diamantina. The locality was

[1] Geognostisches Gemälde von Brasilien, und wahrscheinliches Muttergestein der Diamanten, Weimar, 1822.

[2] Beiträge zur Gebirgskunde Brasiliens, Berlin, 1832, pp. 210, 216.

visited and minutely described by Helmreichen[1] who confirmed Eschwege's views of the genetic relation of the diamond with itacolumite, although he confessed a doubt as to whether the rock, which had a conglomeratic aspect, might not be of clastic origin. Subtantially the same view was taken by Heusser and Claraz[2] who also visited the locality and who considered itacolumite as a quartzose phase of hornblende-schist and attributed the apparent pebbles of the diamond-bearing bed to concretionary action. In 1882, the present writer showed[3] that in the Diamantina region the massive itacolumite of Eschwege really constitutes an independent formation resting unconformably on the upturned edges of a lower series to which the schistose type belongs, and containing elements derived from it, the diamond being probably one of these derived elements. Several of the "dry diggings" of the vicinity of Diamantina were cited as being probably disintegrated masses of this ancient and metamorphosed conglomerate and the Grão Mogol deposit, which was not seen, was referred to as being presumably another example of the same kind. Professor Gorceix who afterward visited the Grão Mogol locality, and who for the Diamantina and other regions accepted my view of the dual character of itacolumite as originally described, agreed with Helmreichen and Heusser and Claraz in uniting the diamond-bearing bed with the lower schistose itacolumite, but, in opposition to their view, he considered the whole series as clastic.[4] The pebbles, or pebble-like bodies of the old writers, of the diamond-bearing bed were thought to be derived elements, while the mica, pyrite and martite of the same rock were considered as authigenic. The question as to which of these two groups of elements the diamond should be referred was, on theoretic grounds, decided in favor of the latter, a view that was rendered necessary by that

[1] Ueber das Geognostische Vorkommen der Diamanten und ihre Gewinnungsmethoden auf der Serra do Grão-Mogor. Vienna, 1846.

[2] Zeitschrift d. deutsch. geol. Gesellschaft, XI, 1859, p. 448; Petermann's Mitth., 1859, p. 447.

[3] Am. Jour. of Sci., 1882, XXIII, p. 97; XXIV, p. 34.

[4] Bulletin de la Societé Géologique de France, XII, 1884, p. 538.

of the unity of the formation, since elsewhere the diamond was considered to be authigenic under different conditions in a different rock referred to the same geological series.

The question of the unity of the section at Grão Mogol and of the clastic origin of at least the upper portion to which, according to all authorities, the diamond is confined, is an important one in this connection, since, if Gorceix's view is correct, it involves that of two widely different modes of genesis for the diamond in the same field. Not having seen the place, it is with considerable diffidence that I venture to contest the views of the eminent authorities who have.

The accompanying figure, reproduced from one of Helmreichen's sketches, gives what seems to me to be a more accurate

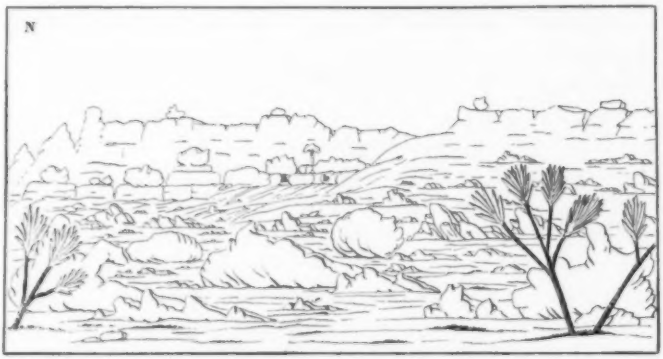


FIG. 1.— Left bank of Corrego dos Bois, near Grão Mogol showing diamond-bearing bed (*a*, *b*). After Helmreichen.

representation of the actual aspect of the exposure than the somewhat diagrammatic section given by Gorceix, and it has the further element of authenticity of representing, at the base of the diamond-bearing bed (*a*, *b*), an apparent unconformability that is not in accord with the theoretic views of the author of the sketch. Numerous cases of a break in the succession have, on close examination, been observed in the Diamantina district that are much less apparent in the topographical features than

the one presumed to be here represented. The two rocks are often found united in the same rock-mass in such an intimate manner that, given the almost perfect identity in aspect and character, one is frequently inclined to doubt the evidence of his senses even after unequivocal proofs of the existence of a break have been detected. Similar cases of a complete blending of his massive and schistose types are graphically represented in other sketches given by Helmreichen which can best be interpreted on the hypothesis of an unconformability. In the present case, it seems to me that, admitting the accuracy of this sketch, the appearance of a break is so evident that before rejecting it most geologists would require much stronger evidence than has yet been presented.

A recent examination of a specimen with an inclosed diamond in the collection of the National Museum at Rio de Janeiro shows that the Grão Mogol rock contains both authigenic and allothigenic elements to either of which groups, leaving out of account considerations derived from other points, the diamond might with equal plausibility be referred. The predominant element quartz,which is presumably allothigenic, has by recrystallization, secondary enlargement, or other process, taken on the aspect of an authigenic element. The mica-like mineral (apparently a clintonite) and the iron minerals, pyrite and martite (magnetite?) are certainly authigenic. Allothigenic elements are represented (leaving out of account the pebbles which are not well defined in the specimen in question) by distinctly rolled zircons. Specimens of typical *schistose* itacolumite, which though not from Grão Mogol may be taken as representing the supposed lower series of that place, present the same mixture of authigenic and allothigenic elements (the latter represented by well rolled zircons) and therefore the same evidence of clastic origin.

As the case stands at present, the evidence from Grão Mogol regarding the genesis of the diamond is inconclusive. The rock, whether one or two series are represented, is a metamorphosed clastic and no decisive evidence can be presented to place the diamond in the class of either the authigenic or allothigenic ele-

ments of this rock. If it belongs to the former class, two modes of genesis must be admitted in the same field in order to reconcile the occurrence with that at São João da Chapada ; if to the latter, the two occurrences can be explained on the hypothesis of a single mode of genesis, but, in case the series proves to be a single one, two periods of formation must be admitted.

For the question of genesis the most significant of the Brazilian localities is that of São João da Chapada near Diamantina

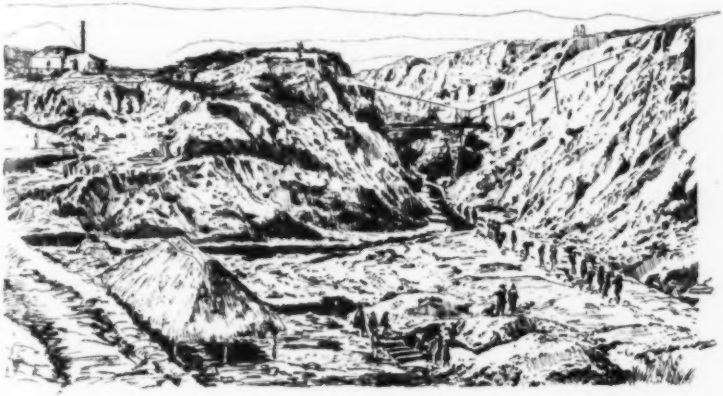


FIG. 2.— Diamond mine at São João de Chapada. The view is looking northward into the Barro mine, which runs into the Duro under the footbridge. The diamond-bearing layers dip into the bank at the right at an angle of about 50°. The open space in the foreground where the wash-house, concentration tanks (*canoas*), and heaps of diamond-bearing earth are located, represent the excavated portion of the left bank containing the upward prolongation of the diamantiferous layers. The wide trench near the pump-house at the left is apparently on the outcrop of one of the layers.

which has never been satisfactorily studied and described. The accompanying view, drawn from a photograph, which is also reproduced in Boutan's monograph on the Diamond in Fremy's *Enclyclopedie Chimique*, shows its character as a great gash resembling a railroad cutting across the crest of a high ridge that forms part of the divide between the waters of the Jequitinhonha and

the São Francisco. Two mines, the Barro (clay) and the Duro (hard), opened on opposite sides of the ridge have come together in the center (the footbridge in the middle ground of the view marks the division) so as to form a continuous cutting nearly 500^{m} long with about 40^{m} of maximum depth. The Barro mine drains to the São Francisco through the Rio Pardo, an affluent of the Rio das Velhas, the Duro to the Jequitinhonha through the Caethé-merim. Both the Pardo and the Caethé-merim, but particularly the latter, are famous diamond streams.

The material exposed in the mines is a soft, soapy clay that is graphically described by Burton[1] as follows. "The material is a hardened paste of clay, whose regular and level stratification argues it to have been deposited in shallow water. The eastern side of the gap is the more ferruginous formation (terra vermelha); on the west it is mixed with beds of white sand. Below one foot of brown soil the argillaceous matter has the usual staining and marbling, glaring white like fuller's earth, with feldspar and kaolin, chocolate-brown or rape-colored with organic matter, blue-green with traces of copper [?], pink and rose purple, and dark yellow with various oxides of iron, especially hematite, and dark steel color with oxide of manganese[?]."

In the whole extension of this immense cutting, nothing approaching the gravel, the usual characteristic of a diamond mine, is to be seen and, with the exception of quartz veins, it is difficult to find a specimen that will resist the pressure of the fingers. The structure as exposed on the sides of the cutting, is much obscured by slides and weather action, and Burton's mistake of horizontal stratification and other indications of a shallow water deposit (thus corresponding, except in the character of the material with the other diamond deposits of the region and with the preconceived ideas of his informants, the miners) was a natural one on the part of a non-geological observer. The true nature of the clay as a decomposition product of schistose rocks had already been clearly recognized by Heusser and Claraz, who first described the mine and who iden-

[1] The Highlands of the Brazil, London, 1869, II, p. 129.

tified the original rock type as hornblende-schist,[1] giving the impression that the diamond occurred throughout the whole mass. The limitation of the diamond to certain streaks, or layers, was recognized by Burton, who gives the following excellent description of them.

"The richest lode (*corpo*) is No. 3, or the highest. The strike of the ribboned clays is north and south, bending [dipping] eastward. The lode inclines towards the higher grounds, and thus the owner hopes to find the gem-bearing strata spreading over the crest or watershed ridge which forms his property. Through the ferruginous sandstone (*borra*) and the white feldspathic matter run dikes and lines of fragmentary rock crystal, sometimes fibrous like arragonite, and often finely comminuted. Large pieces of imperfect specular iron and thin strata of quartz, yellow and brown at the junction, thread the argile, and I was shown a specimen of fine sandy conglomerate, blackened and scorified by the injection of melted matter.[2] The characteristics of this upper lode are a dryer clay, silica, a trace of copper [a green silicate of the nature of chlorite or serpentine?], of iron cement, and of Canga in small pieces; when the specular iron is in large pieces and abundant the rock is rich in gems. Its 'agulhas' [rutile] are iron-like bundles of needles welded together by intense heat; some are double, the fibers coming at obtuse angles. The 'Agulhas Cor de Ouro' have a burnished coppery surface, whence the name. Throughout all these corpos the diamonds are small, averaging perhaps a little under one grain or 64–72 per oitava; they are mostly crusted superficially

[1] Although Rose failed to find hornblende in the material submitted to him by Heusser and Claraz, it is possible that this idea was not entirely without foundation. Throughout the whole region traversed by them, intercalations and dikes of amphibolic and pyroxenic rocks are frequent in the schist series and generally they are the only ones of the not distinctly quartzose rocks whose original composition can be readily determined. As they usually give green decomposition products the staining referred by Burton to copper may, with considerable plausibility, be attributed to them. To judge from other exposures, the absence rather than the presence of such rocks at São João da Chapada would be a motive for remark.

[2] Probably a "Canga" or mass of sand or clay cemented by limonite common in such deposits.

with a light green tinge. Lower down we came to the middle or second body. Here the 'tauá' (feldspathic clay) was stiff and sandy, marbled with a fat, blue, muddy marl, which leaves upon the fingers a greasy, steely streak. It also yields a dark olive-green argile harder than the rest; like all the others it has consistence in situ, but when removed it crumbles to pieces after drying. Lieutenant-Colonel Brant gave me from this corpo a fragment of hard, large-grained clay, reddish colored with oxide, and showing a small brilliant imbedded in it. We then descended to the lowest formation. Here the clay contains very little sand, and much stained; the colors are white and blue, red and yellow, rosy, spotty, and in places dyed as with blood. Here also are found the 'Agulhas' in streaky bundles of iron-like asbestos. The sole of the pit is uneven with working, and in places 'horses,' 'old men,' and long walls of stiff clay have been left standing amongst the holes and gashes. From this point the several lodes are distinctly traceable in the walls of the basin." A more technical description is given by Gorceix[1] as follows: "One of the beds of bluish black color is composed of clay impregnated with oligiste in small fragments with rutile and anatase; the second of lithomarge with entire crystals of quartz having the same aspect as those of the topaz mines [near Ouro Preto]; the third and most important, with a thickness of more than 1.50^{m}, is composed of a series of beds of mottled clay. The stratification planes, parallel to those of the quartzites, are still clearly visible; the layers are undulated, folded like those of the intact schists that are found a few meters distant. Fragments of schist still almost intact occur in the midst of the clay. These beds of clay are traversed by small veins of quartz, granular or in bipyramidal crystals, oligiste and rutile, showing no signs of wear. Octahedral oligiste is found in certain points in extreme abundance impregnating the rock; in other points it is substituted by ordinary oligiste. The aspect of the gravel resulting from the washing of this clay is entirely different from that of the alluvial deposits, though composed of the same elements. The

[1] Comptes Rendus, 1881.

diamonds themselves of this region are easily distinguished from those of the rolled deposits by their rugose faces, sharp angles, and superficial greenish blue coloration."

On the occasion of my visit in 1881 only fallen masses that appear to represent the bodies 3 and 2 of the above description were to be seen, the operations of the mine being at that time suspended. One of these masses was so evidently a section of a vein that the conclusion was announced[1] that at São João da Chapada the diamond is a vein mineral accompanying quartz and an argillaceous rock of indeterminable character in the series of metamorphic schists. This conclusion was subsequently fully confirmed by Gorceix as the result of prolonged prospecting operations,[2] and thus one mode of occurrence of the diamond in the matrix was clearly established which, in appearance at least, is widely different from that at Gão Mogol and at Kimberley.

In the various papers by Gorceix and myself the schistose series of the gold and diamond region of Minas Geraes, in which the diamond occurs at São João da Chapada, is assumed to consist essentially of metamorphosed clastics, though no direct proof of this assumption is given. As regards the diamond-bearing layers, they are called veins, but no definite opinion regarding their mode of origin is expressed. For the question of the genesis of the diamond both these points are important.

All writers on the geology of this region are agreed that the characteristic formation is a great series of phyllites, quartz schists (itacolumite), iron-mica schists (itabirite), and limestone, and that this series constitutes a geological unit. This last point is assumed rather than proven, since there may be a break in the succession which has thus far escaped observation, just as that above indicated between the upper and the lower itacolumite was overlooked, or disregarded, by the older writers. For the purposes of the present discussion, however, it is of little conse-

[1] Am. Jour. Sci., 1882, Vol. XXIII, p. 97.

[2] Comptes Rendus, No. 25, 1881. Am. Jour. Sci., 1882, Vol. XXIII, p. 97, and Vol. XXIV, p. 34.

quence whether the series as here limited by the exclusion of the massive itacolumite of Eschwege is a simple or a composite one, since the evidence as to origin applies very directly to the beds in question exposed in the diamond mine itself.

As recent studies in various parts of the world on schists of doubtful character have proved that the schistose structure is not, as was long supposed, an unequivocal proof of a clastic origin, an attempt has recently been made to find in the rocks themselves internal evidence for or against the hypothesis of such an origin. As is well known, most of the mineralogical elements of a metamorphosed rock, whether clastic or otherwise, are authigenic; others which in certain cases may be presumed to be allothigenic may have been broken up, recrystallized, enlarged by secondary additions,[1] or etched, so that all traces of the original worn surface of clastic grains may have been lost or so obscured as not to be recognizable with certainty. The hopes of finding such internal evidence are therefore limited to the rare accessories, and among these practically to zircon, not only on account of its almost universal distribution in sedimentary deposits, but also of its resistance to chemical changes. In the examination of the heavy residues of a considerable number of the rocks of the series in question, it was found that in rocks of their character and degree of metamorphism, zircon is the only element that can be depended upon to give unequivocal evidence as to the mode of origin. All the other constituents, principal or accessory (quartz, mica, iron, and titanium oxides, tourmaline, disthene, etc.), present the fresh appearance of authigenic elements, as most of them doubtless are, though in some cases this appearance may be due to the fragmentation, regeneration, or etching of original clastic grains. On the contrary, the zircons in the considerable number of residues examined have failed to show evidence of secondary modification by any of the processes

[1] As will be shown elsewhere, tourmaline may be regenerated by secondary enlargement in the same manner as in the well-known case of quartz. A remarkably fresh appearance of the surfaces of quartz grains, due to etching, is noticeable in the washings from the clays of São João da Chapada, but it is to be presumed that this is a phenomenon of decomposition rather than of metamorphism.

mentioned so that, when present, their evidence is positive when they show distinct signs of wear, doubtful when these signs are absent or dubious.[1]

Applying the zircon test to the material at hand from the São João da Chapada mine the evidence for the clastic origin of the greater part of the original rock types from which the clays are derived has proved to be unexpectedly satisfactory. A number of samples of typical clays, including some reputed to be diamantiferous, afforded zircons which in abundance, size, and amount of wear, are comparable with those of the granular quartz rock (itacolumite) that occurs above the schists in the immediate vicinity of the mine. To judge from the number and character of the zircons, these samples represent original grits rather than more purely argillaceous material as was supposed from their present character and appearance. This conclusion is confirmed by the amount and size of the quartz grains (beautifully etched) that are also separated by the washing of the clay. In a miner's concentrate representing mixed material, fresh, prismatic glassy zircons occur mingled with the ordinary rounded, reddish clastic type indicating that other types of rock, presumably eruptive, may be represented among the clays. For the present discussion, however, the essential point is that the generality of the zircons of the clays are worn, thus confirming the assumption, based on stratigraphical evidence, that the clays of the mine represent a series of schists of which the predominant types are of clastic origin. This conclusion, however, does not exclude the possibility of subordinate intercalations,

[1] The rounding of the angles alone cannot be taken as an unequivocal sign of wear, as it is often an original feature of the zircons of undoubted eruptive rocks. When the angles are rounded by attrition the faces are also dulled in a manner that is readily distinguishable from that produced by malaconization. Undoubted clastics occur in which the signs of wear of the zircons are inappreciable, either because the amount of transportation has been too small or the material too fine to produce them, or because they have been involved in other elements, as in the case of arkose and tuffs. In the case of argillaceous rocks the rarity and minuteness of the zircons may be an argument in favor of a clastic origin even when they show no distinct signs of wear, but the evidence is not conclusive as they are minute and rare in some eruptives as well.

or injections, of eruptive origin, which, judging from evidence elsewhere, are rather to be expected than otherwise in a cutting like that of São João da Chapada. For the question of the genesis of the diamond this hypothesis is of prime importance and the evidence thus far available for or against it will now be examined.

Of the three diamond-bearing bodies described by Burton and Gorceix only two were seen by me. The masses shown me were displaced by landslides, but, as nearly as can be determined, they represent the middle and lower bodies of Burton.

The mass supposed to represent the lower body of Burton and the mottled clay of Gorciex consisted of a considerable rectangular block of quartz, with plates of specular iron, and with laminated clay representing the decomposed country rock adhering to it on one side. On the other side was a mass of friable structureless reddish clay, sharply defined on the side opposite the quartz from the harder laminated clay of the decomposed country rock, which is here also reddish, but of a different tint and aspect. The whole appearance of the mass was that of a vein with sharply defined walls, and it was so described on account of the quartz, though, as the earthy portion was referred to a decomposed rock of undetermined character, the term dike might have been employed with equal propriety. The earthy diamond-bearing mass was shown to consist of an argillaceous portion stained with iron oxide and a sandy portion with quartz and tourmaline. The heavy residue which has since been separated and examined consists principally of aggregations of specular iron and of a micaceous mineral representing some altered silicate with a great abundance of microscopic brown tourmaline. Yellowish, burr-like aggregates of anatase are also abundant, while rutile is comparatively rare, as are also grains of disthene. All of these minerals are evidently authigenic. The rare grains of zircon are in part distinctly worn, in part with the fresh appearance of an authigenic element. A few grains of staurolite also occur, and these are, for the most part, rounded, giving them a worn appearance,

but as some of them are distinctly etched, it is thought that this aspect may be due to etching rather than to attrition. Unfortunately, it is not absolutely certain that the zircon and staurolite may not have been introduced from a foreign source, as at the time the washing was made the extreme care now found necessary to avoid admixture was not observed. On the assumption that the residue is a pure one (as it is believed to be),[1] the interpretation would be that the original vein material contained primary tourmaline and zircon with iron and titanium minerals that have furnished material for secondary hematite, anatase, and rutile, and that the accompanying schist contained clastic zircons, staurolite that is authigenic if the rounding of the grains can be attributed exclusively to etching, and disthene. The hypothesis that best suits these conditions is that of a granitic (pegmatitic) vein accompanied by phenomena of contact metamorphism.

The mass that was shown to me as representing the Barro Preto (black clay, middle body of Burton) had the characteristic of a bed rather than of a vein. The clay is well laminated, ribboned with fine regular alternating streaks of white and black, the latter composed mainly of a fine powder of hematite. The residue, freed from clay, shows a great abundance of black hematite, so finely divided that much of it floats away in the washing, a moderate amount of etched quartz, a small amount of tourmaline in coarser grains than in the body above described, and a comparative abundance of rolled zircons, which appear also to have been somewhat malaconized. The titanium minerals, rutile and anatase, are absent, or extremely rare. A sample subsequently received as representing the same body agrees substantially with the above, except in the greater abundance of quartz and the absence of tourmaline. All these indi-

[1] In the case of an admixture, rounded staurolites and fresh disthenes are not the minerals that might be expected to be introduced in the residue through lack of care in the preparation. Two or three grains of monazite were found that were certainly introduced by accident, but this very circumstance gives confidence in the general purity of the residue, as the much more abundant disthene and staurolite are not its usual associates.

cations point to an original bed of sandy ferruginous shale rather than to a vein.

The third body, not seen by me, is, according to both Burton and Grociex, characterized by a white feldspathic clay, kaolin, or lithomarge, with crystals of quartz and specular iron. Specimens exactly corresponding to this description were kindly furnished by Dr. Thomassi Bezzi, who collected them with the assistance of the owner of the mine, so that their authenticity is undoubted. Two specimens representing the Barro and the Duro mines are practically identical. In both a mass of snow-white, structureless clay, with nests of quartz crystals and specular iron, has adhering to it colored laminated clays. The contact between the two, sharply defined by the strongly contrasted coloration, is in part linear, in part irregularly undulated, but without appearance of graduation from one to the other. Irregular stringers of the white clay penetrate the mass of the colored, and irregular masses of the latter are inclosed on the former. The whole appearance of the contact is that of a vein or dike, represented by the white clay, with stringers and inclusions of the country rock. The contrast between the heavy residues of the two kinds of clay is as striking as that of their coloration and general appearance. Corresponding quantities taken at a distance of a few millimeters from each other or either side of the contact gave very different residues, both as regards quantity and mineral composition. That of the white clay is extremely small, consisting, aside from rare grains of quartz and specular iron that apparently come from segregations rather than from the body of the clay, exclusively of delicate needles of yellow rutile, the *Agulhas cor de ouro* (golden needles) mentioned by Burton. The residue from the colored clay is, on the contrary, abundant, consisting, after the separation of the quartz (beautifully etched) and iron oxide, of rolled zircons, anatase, tourmaline, and iron-stained earthy grains of rudely prismatic form that evidently represent a decomposed silicate, possibly staurolite. This last is a characteristic residue of a metamorphosed clastic rock, and as tourmaline and anatase seem to be

present in abnormal abundance, contact metamorphism is strongly suggested. The residue of the white clay, on the other hand, gives no indication as to its origin, since the only characteristic accessory found in the small quantity available for washing is rutile, which is so widespread and varied in its mode of occurrence and association as to be indeterminative. One of the washings from the colored clay gave two types of zircons, the usual round, much worn reddish ones, and less worn whitish elongated prisms. The latter resemble those already mentioned as occurring (with a fresher appearance, however,) in a miner's concentrate, and still more closely those of a partially decomposed rock from the Sopa mine in the neighborhood (where lithomarge also occurs, but is not known to be diamantiferous), which strongly resembles the European "porphyroid," and is either metamorphosed arkose or porphyry, probably the former.

This white clay, in the character of its material and of its contacts, and in the lack of characteristic clastic elements, is strongly suggestive of the so-called pegmatite veins that are of frequent occurrence in similar formations and under similar conditions. The quartzose character of some of the veins, or parts of veins, is not inconsistent with this hypothesis, as the intimate relations and interdependence of quartz and pegmatite veins are well known. The indications furnished by this body are therefore in accord with those of No. 1—that is to say, that the vein matter was probably originally pegmatitic, and that it was accompanied by phenomena of contact metamorphism.

So far as can be made out from the observations thus far made on material the most unsatisfactory that can be imagined (foliated and highly modified by dynamometamorphism and afterwards totally decomposed so as to present, in its present state, one of the most intricate problems of mud geology), the most plausible hypothesis as regards the various clays of the São João da Chapada mine is that they represent an original group of phyllites of varied character, but principally, if not exclusively, of clastic origin, threaded with veins of pegmatite.

The possibility of an admixture of originally eruptive elements in the phyllites themselves is, as already noted, suggested by the supposed copper staining of Burton and also by the harder olive-green clay that he mentions as occurring with the second body. The only rock specimen that has come to hand from the mine is a small fragment of a sericitic schist that, aside from a very fine dust of hematite, gives no residue whatever, and which may be suspected to be a metamorphosed eruptive.

On the hypothesis of the original essentially pegmatitic character of the diamond-bearing bodies of São João da Chapada three important questions arise which can only be solved hypothetically. What was the original type of the pegmatite? Is it eruptive or secretionary? Do the diamonds belong to it or to the country rock in its immediate vicinity, and perhaps modified by it, or to both?

Bodies of pegmatite are quite common in the older rocks of Brazil, both in the diamond regions and elsewhere, occurring not only in the gneiss and granite, but in the schistose series as well. Those that have been examined are dike-like in their mode of occurrence and granitic in composition. They are almost universally decomposed, affording a pulverulent kaolin, not the indurated type of lithomarge. Their residues are usually abundant and typically granitic, representing more particularly the type of the muscovite granites, consisting of zircon, monazite, and almost invariably xenotime. All of these characteristics (which, however, may not be essential) are lacking in the supposed pegmatitic clay of São João da Chapada, in which only the presence of quartz is suggestive of granite affiliation. On the other hand, however, they are compared with great propriety by Gorceix with the topaz-bearing clays of Ouro Preto, and topaz is generally regarded as a granitic mineral. Topaz has not been reported from São João da Chapada, but in one washing from a mixed sample of the clays a minute grain was observed that in form, optical properties, and specific gravity seems to agree with that mineral. The other known types of pegmatite—those affiliated with syenites, diorites, and gabbros

—have not as yet been definitely identified in Brazil, though they doubtless occur. The apparent absence (or extreme rarity) of zircon may perhaps be taken as indicative of gabbro, and considerable masses of this type of rock, to which the supposed pegmatite might be referred as apophyses, are known to occur in the diamond region. So far as known, however, this is the utmost limit in the direction of basic rock types to which one can go, even hypothetically, in seeking the probable original type of this supposed pegmatite.

The question of the eruptive or secretionary origin of pegmatites has long been a subject of discussion among geologists, and eminent authorities can be cited in favor of either view. The recent studies of Lehmann, Brögger, Williams, Crosby, and others seem to have clearly established that most if not all of them are essentially eruptive masses, though possibly modified in some way by aqueous agencies. Even before becoming acquainted with the literature of the subject this view had seemed to me to be the only acceptable one as regards the typical pegmatites of Brazil. The extension of it to such problematic occurrences as the diamond-bearing bodies of São João da Chapada and the topaz-bearing bodies of Ouro Preto cannot as yet be fully established on account of the lack of complete studies in the field and the decomposed condition of the material. Aside from the general analogy that they present with typical pegmatites, nearly all the criteria given in the recent papers by Williams and Crosby and Fuller in support of the hypothesis of eruptive origin can be cited in favor of the same hypothesis as applied to these bodies. If, as is suspected, they present phenomena of contact metamorphism, a crucial test can be applied through the study of the heavy residues of the enclosing schists at different distances from the contact. This, however, involves field studies that for the present cannot be undertaken. As the case stands at present the hypothesis of an eruptive origin, though not fully proven, is by far the most probable.

The response to the third question is still more unsatisfac-

tory than those to the other two. At the time of Burton's visit the most typical pegmatitic body was regarded as the richest of the Duro mine and in his description of the Barro mine he states that the white clay (called *diz*, or chalk, by the miners) served as a guide to the diamond formation. It is by no means certain, however, that the diamonds actually occurred in it rather than in the adjacent colored clays in contact with it and for which it serves as the most apparent guide. In the specimens at hand the part considered as the contact zone is mineralogically the richest, and it may be suspected that the diamonds occur in it rather than in the white clay. The lower body reputed to be the richest at the time of Gorceix's visit is, according to his description and that of Burton, much less decidedly pegmatitic in aspect and the part seen by me seemed to be a decomposed dike with no apparent suggestion of pegmatite. The part of this body indicated to me as diamantiferous belongs certainly to the supposed dike and not to the contact zone. The other body, the Barro Preto, seems, according to the descriptions and the part seen by me, to be a specialized layer of the phyllites the relations of which to the pegmatites (if it has any) are not clear. In short the question as to whether the diamond occurs at São João da Chapada exclusively in the supposed pegmatitic bodies, in the contact zone of said bodies, in layers of the phyllites more or less removed from such contact zones, or in all, must remain an open one.

As the case stands at present the indications seem to be rather in favor of the hypothesis of the formation of the diamond in the phyllites, the presumptive agent being the supposed eruptive which in some of its phases presents a pegmatitic character. This involves, presumably though perhaps not necessarily, the supply of the necessary carbon from the phyllites themselves, but as the series is known to include in many places graphitic members such a supply may reasonably be predicted at São João da Chapada. Moreover the evidence from Kimberley, where, according to Launay (*Les Diamants du Cap*), the rock considered rich only yields one part of diamond to 3 mil-

.ion to 36 million parts of rock, indicates that the amount required is so infinitely small that few rocks can be conceived that may not contain, in some form, the necessary supply of carbon. The amount of this element that presents itself in the form of carbonates in the decay of many rocks, that in their sound condition are not recognized as containing it in any form, is far in excess of that here indicated as necessary. In this connection it may be remarked that the hypothesis that attributes a preponderant importance in the genesis of the diamond to the carbonaceous shales of the upper part of the Kimberley section, is subject to the criticism of furnishing a preposterously enormous superabundance of raw material.

The three localities above discussed offer no certain indications of more than one mode of genesis of the diamond in Brazil. The occurrences at Grão Mogol and Sãn João da Chapada can very easily be brought into line on the hypothesis, which has much in its favor that at the former place the diamond is an allothigenic mineral derived from deposits similar to those at the latter. For São João da Chapada and Agua Suja the comparison presents no difficulty if the diamonds at the last place are assumed to come, as is quite possible, from the underlying schists. If, however, they are genetically related to the later eruptive series, the hypothesis of a substantially similar mode of genesis requires that the predominant factor be an eruptive rock, which may vary greatly in its mineralogical character and mode of occurrence.

As compared with the Kimberley occurrence that of São João da Chapada seems at first sight to be characterized by an almost absolute lack of analogies. Until quite recently the only known feature at Kimberley offering some remote resemblance to the Brazilian fields was the presence of a quartzite in the lower part of the section. This resemblance is somewhat increased by the later developments as metamorphic schists appear mingled with the quartzite in the lower levels of the deep shaft (see section on p. 137 of Launay's *Les Diamants du Cap*). For the present the information regarding these lower

rocks is very meager, and further developments must be awaited to determine whether or not this resemblance has any special significance. Another point in common that may prove to be of greater significance than at first sight appears is the occurrence mentioned by Carvill Lewis[1] of tourmaline and disthene which seem to be formed by metamorphic action about inclosed fragments of schist. It is true that disthene is not a specially characteristic mineral at São João da Chapada, but it is extremely widespread and abundant in the schist series in which the mine is excavated, and of the aluminous silicates, is the most constant and characteristic of the associates of the diamond in the Brazilian alluvial washings. The significance, if any, of its persistent association with the diamond (now verified at Kimberley) is that of a mineral characteristic of the metamorphism (by contact or otherwise) of argillaceous rocks.

In order to bring the Kimberley and Brazilian mode of occurrence into line as phases of a single mode of genesis, it seems necessary to put aside the idea that the recent interesting experiments on the artificial production of the diamond afford a solution of its terrestrial origin, and that the Kimberley type of rock and mode of occurrence are essential features. Presumably also the genesis must be sought in the rocks affected by the eruptive masses rather than in those masses themselves. There are still many obscure points about both places, and until these are cleared up no satisfactory comparison can be made. At São João da Chapada there is little prospect of working being resumed so that no additional light is to be expected from there, but at Kimberley the workings may ultimately reach a depth that will give a complete solution of the problem for that place and mode of occurrence. When this occurs, if it is verified that the ultra basic type of eruptive rock, brecciated structure, and slot-like mode of occurrence are necessary features, the Brazilian occurrences must be put into another category.

ORVILLE A. DERBY.

SÃO PAULO, November 29, 1897.

[1] Papers and notes on the genesis of the diamond.

THE GLACIATION OF NORTH CENTRAL CANADA.[1]

I WISH very briefly to place before you a statement of what would seem to me to have been the conditions that prevailed during at least part of the glacial period in the great Central Plains region of Canada, but before going farther I take great pleasure in acknowledging my indebtedness to Professor Chamberlin, Mr. Warren Upham, and many other glacial geologists of the United States, whose work is so closely connected with mine, and who have so clearly expounded many of the principles on which my explanations are based. It is an especial satisfaction to me to feel that the results of my investigations accord so well with theirs.

In the preparation of the slides here shown I have freely used the published works of these geologists, and of Dr. Dawson and Messrs. McConnell, Low, and of our own geological survey, when depicting the conditions that prevailed in those portions of the country which have not come under my own personal observation.

That portion of Canada, to which I propose to refer for a few moments, lies between west longitudes 85° and 130°, and north latitudes 49° and 69°; or perhaps it may be more intelligibly located as being bounded on the east by the west coast of Hudson Bay, and a prolongation of the same line southward, and on the west by the Rocky Mountains, the average distance between these two lines being about 1100 miles; on the south by the international boundary, and on the north by the Arctic Ocean, which are an average distance apart of 1400 miles, giving a total area of about 1,500,000 square miles.

This vast region has a remarkably even surface contour, with a mean elevation above the sea of about 1200 feet, and slopes gently from the foot of the Rocky Mountains northeastward to

[1] Read at the Toronto meeting of the British Association, August 1897.

Hudson Bay. The contour lines here shown are respectively 600, 1500, and 3000 feet above the sea, and from the 3000 feet contour line the surface has an average slope of a little less than three feet to the mile. This slope is of course not quite regular, being broken by hills and valleys, and occasionally the country rises for some considerable distance in the opposite direction, but on the whole the general decline is very well marked, and no high mountains break the general monotony of the landscape.

In the more northern portion of the region are the treeless plains, or "Barren Lands," extensive level or undulating grassy plains, with a mean summer temperature below 50° F., and with a frozen subsoil which prevents the growth of trees. South of this is the great forest region, the home of the chief fur-bearing animals of Canada, and still farther south are the plains or prairies, which many of you will probably cross on the transcontinental railways after the close of this meeting.

It would be beyond the scope of this paper to discuss the question of rainfall, but suffice it to say that the humidity of the atmosphere decreases from the seacoast inland, and while the Barren Lands are kept constantly wet by fogs and drizzling rains, the air over the prairies is very dry, and licks up rapidly any moisture that may be lying on the surface of the ground.

As you will see from the handbooks prepared for the use of the members of the association, the northeastern part of this region is underlain by crumpled and distorted Archæan rocks, whose surface has, even in pre-Cambrian times, been reduced to an undulating plain, with very slightly accentuated contours. On each side of this elongated area, or low central ridge, of highly altered Archæan rocks, are flat-lying limestones, sandstones, and shales, varying in age from the Cambrian up to the Tertiary, and separated by several erosion intervals, which, with the water-deposited strata, would indicate a gradual rising and lowering of the land along a line parallel to the present Archæan outcrop. From a study of the Rocky Mountains, and the mountainous region of British Columbia,[1] Dr. Dawson has shown rea-

[1] On the Later Physiographical Geology of the Rocky Mountain Region in Can-

sons to believe that the oscillations of the land were exceptionally rapid in early glacial and immediately preglacial times. But it would seem probable that the drainage has always followed the main valleys which still trench the surface, running more or less at right angles to the mountains.

The pre-Cambrian valley of Chesterfield Inlet, extending eastward towards Hudson Strait, and westward towards Great Slave Lake, and the post-Cretaceous valley of the Saskatchewan, extending towards the lower valley of the lower Nelson River, and many other valleys running more or less parallel to these, go to prove the general correctness of this statement.

In the opinion of the writer almost all of this country was overspread by the Keewatin glacier, which centered in what is now the comparatively low country west of Hudson Bay. Evidences of its presence may be seen almost everywhere in striated rock surfaces, giant's kettles, widespreading sheets of unstratified till, stratified inter-till deposits, moraines, eskers, and transported bowlders.

The causes of the great cold of the glacial epoch are yet enshrouded in mystery, and the most that has been suggested is that if such and such things had been so, if the land had been higher here or lower there, ice would have accumulated in northern latitudes, but as yet there is little or no proof that such conditions did exist. At present it would appear to be much more satisfactory to spend our energies in endeavoring to follow up the traces left by the glaciers and lakes of the glacial epoch, and to first determine the conditions that existed at one time, or the order in which certain conditions existed, rather than to devise elaborate theories to account for conditions that may never have occurred. When the country has been thoroughly examined, and the glacial deposits, striæ, etc., are well known, the proximate causes of these phenomena will in all probability be easily determined.

The information with regard to the conditions of glaciation

ada, by GEORGE M. DAWSON, Trans. Roy. Soc. Can., Vol. VIII, Sec. 4, pp. 1–74. Ottawa, 1890.

unearthed is undoubtedly but a very small portion of what will be known when our country is more fully explored, for compared to the vastness of the field and the probable extent of the harvest of knowledge, the harvesters are indeed very few. The observations here discussed have been made by the writer during the past thirteen years, but as individual records are difficult to grasp and remember, I have attempted to connect them in such a way, and to bring them graphically before you, so that you may form a clear idea of the results that have been attained, and at the same time I shall endeavor to state very briefly some of the evidence on which those results have been based, so that you may distinguish between the records and the connecting theories.

Up to the present time three great continental glaciers have been recognized in Canada, viz., the Cordilleran, which covered the western mountains and their intervening plains, from latitude 49° to latitude 66°; the Keewatin, which covered the great plains east of the mountains; and the Labradorean, which spread over northeastern America from a center in Labrador.

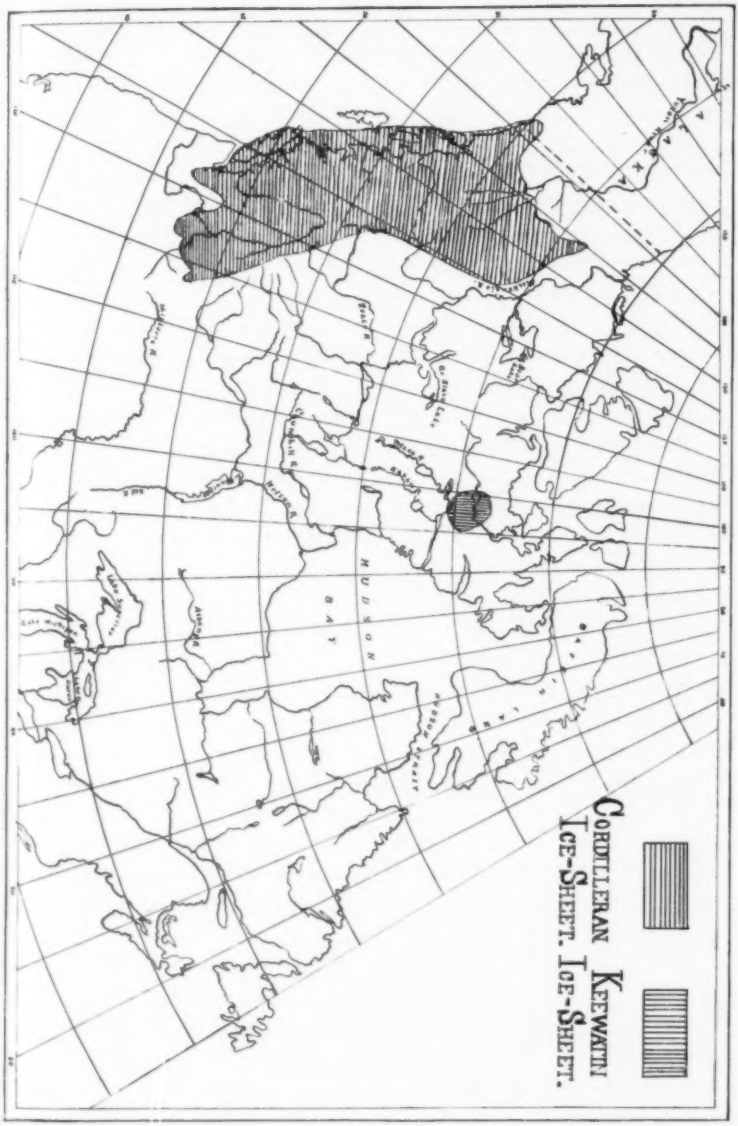
The earliest till as yet recognized in Western Canada, east of the Rockies, has been called by Dr. Dawson the Albertan Deposit,[1] and has been shown by him to have been formed by tongues of the Cordilleran glacier, which extended outwards towards the plains through the transverse valleys of the Rocky Mountains.

The illustration shows the greatest extent of this Cordilleran glacier as defined by Dr. Dawson.

From the fronts of these glacial tongues streams rushed eastward, carrying with them large quantities of coarse detritus which were soon deposited in the bottoms of the valleys as beds of coarse, well-rounded gravel, called by Mr. McConnell the Saskatchewan gravel, this gravel and the Albertan till being directly traceable into each other.

The Cordilleran glacier then withdrew; but whether it entirely

[1] Glacial Deposits of Southwestern Alberta, by GEORGE M. DAWSON and R. G. MCCONNELL, Bull. Geol. Soc. Am., Vol. VII, p. 66, Rochester, 1895.


CORDILLERAN ICE-SHEET.
KEEWATIN ICE-SHEET.
HUDSON BAY

disappeared, or merely confined itself to the west side of the Rocky Mountains, I do not know, but at all events it seems to have ceased to be an important element in molding the features of the plains.

After the withdrawal (or possible disappearance) of the Cordilleran glacier, the Keewatin glacier overspread the country, radiating outwards from a center probably lying somewhere between Doobaunt and Back rivers. The till formed during this period, which is probably synchronous with that of the sub-Aftonian period of Professor Chamberlin, may be easily recognized in the scarped banks of many of the streams in Alberta, where it overlies the Saskatchewan gravel of the Albertan period. It is composed largely of material derived from the underlying Cretaceous and Laramie rocks, but at the same time it contains a considerable quantity of other material transported from a distance, some of which, consisting of granite, gneiss, quartzite, and similar rocks, has been derived from the Archæan nucleus to the northeast, while some has been derived from the Cambrian sandstones, and Cambro-Silurian and Silurian limestones that extend around the edge of the Archæan.

After the deposition of the sub-Aftonian till the Aftonian period of deglaciation set in, during which the Keewatin glacier became greatly diminished, and interglacial deposits were laid down, both in extraglacial lakes, and in lakes and swamps at some distance from the face of the glacier. How far the foot of the glacier withdrew in this interglacial time I do not know, but I am inclined to think that most of Manitoba still remained covered with ice, for in the western part of that province I have not been able to find evidence of more than one main Keewatin interglacial period, which is probably later than the Aftonian period.

After this period of diminution the Keewatin glacier again began to increase, and it spread southward and westward until it had reached about the same limits that it had reached during the sub-Aftonian period, and had spread another sheet of till over the earlier till and subsequent interglacial deposits. The

period of this advance of the ice would seem to correspond to the Kansan epoch, as at present understood by American geologists. This till is very similar to that below it, but the material of which it is composed is more highly oxidized and decayed, and fragments of soft, brittle rocks, such as lignite, are much less common in it than in the lower till.

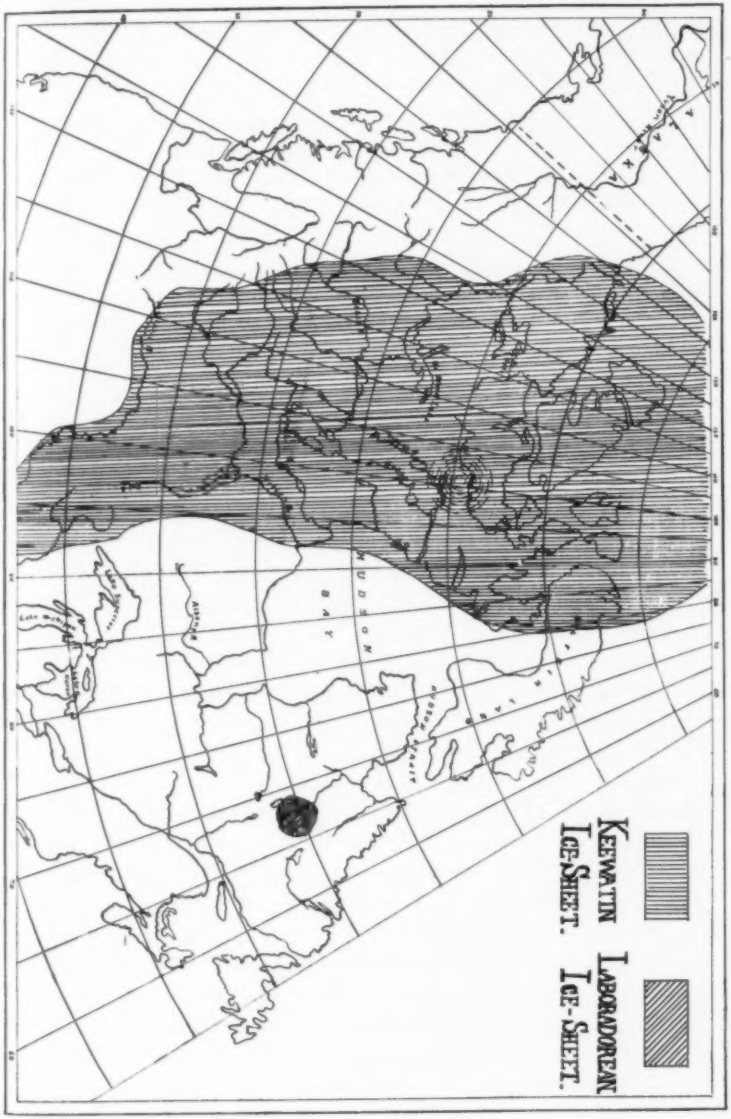
Both during the Kansan and sub-Aftonian epochs, extraglacial lakes of greater or less size doubtless existed, and any material brought down into them by the glaciers would doubtless have been scattered over their floors or along their sides. Thus bowlders were probably carried some distance beyond the extreme limits to which the glaciers themselves reached, as, for instance, on some of the terraces near, and at the foot of the Rocky Mountains where Dr. Dawson has recorded the occurrence of numerous transported bowlders. Where these lakes existed terminal moraines would also not be found, and thus the absence of terminal moraines may often be explained in places where we should otherwise expect to find them.

Striæ have not been recognized in this western district, for the soft Cretaceous rocks are not suitable for their preservation, but the older and harder rocks, nearer the center of the glaciated area, are everywhere scored with glacial markings. Around the periphery of this area underlain by harder rocks I have not been able to recognize more than one set of striæ referable to the Keewatin ice-sheet or rather more than one direction of striation, but nearer the glacial center several sets may be distinctly seen. Along the Doobaunt River above the Forks the oldest of these point southward, probably running outwards from a center between Doobaunt and Back rivers. These I have associated with the earlier stages of the Keewatin glacier, though I have no direct evidence to offer on that point, except that they are the earliest of four different and distinct sets of glacial striæ.

The accomplished geologists who have worked in the United States, near the headwaters of the Mississippi River, have found that there was an extended epoch of deglaciation after the deposition of the Kansan till, and I would assign to this interglacial

epoch a line of division in the till of western Manitoba, along which stratified deposits may occasionally be seen, but which is strongly marked in several places by beds of bowlders that have been sunk in the surface of the earlier till, and being there held firmly in place have been smoothed and striated by the later glacier which passed over them. How far the Keewatin glacier retreated during this second epoch of deglaciation is not known, but it is not improbable that it withdrew far north of the present northern boundary of Manitoba.

After this interglacial epoch the Keewatin glacier again began to increase, though its center of dispersion had gradually shifted southeastward until it now rested over the country between Doobaunt and Kazan rivers. From this center it flowed outwards in all directions, and its striæ may be seen on most of the rocky knolls throughout that whole northern country, running southward towards Lake Winnipeg, westward towards the Mackenzie River, northward towards the Arctic ocean, and eastward towards Hudson Bay. Everywhere the smooth-faced hills give evidence of its presence, and even in the absence of striæ, the evenly-rounded surfaces facing the glacial center, and the broken, craggy hills looking in the opposite direction, furnish convincing evidence of the direction of flow of the ice. As the glacier advanced southward it came in contact with the high escarpment of Cretaceous shales forming the Porcupine and Duck mountains in Manitoba, and part of it was diverted to the east of south along the great valley of Lake Winnipeg. This lobe appears to have extended southward into Minnesota, Dakota, and Iowa, and to have deposited a till which probably corresponds to what Professor Chamberlin has called the East Iowan Formation. The Palæozoic limestones of western Manitoba have been beautifully scored by the markings of this glacier, and its grooves and striæ were detected in many places around the shores of Lake Winnipeg. East of the shores of Lake Winnipeg the exposed surfaces of the Archæan rocks were carefully searched for this set of markings, but none could be detected. It therefore seems probable that the eastern edge of this lobe or portion of


KEEWATIN ICE-SHEET.
LABRADOREAN ICE-SHEET.
HUDSON BAY

the Keewatin glacier did not extend very far east of the present eastern shore of Lake Winnipeg, and it is also probable that throughout its advance there was a free drainage eastward into Hudson Bay.

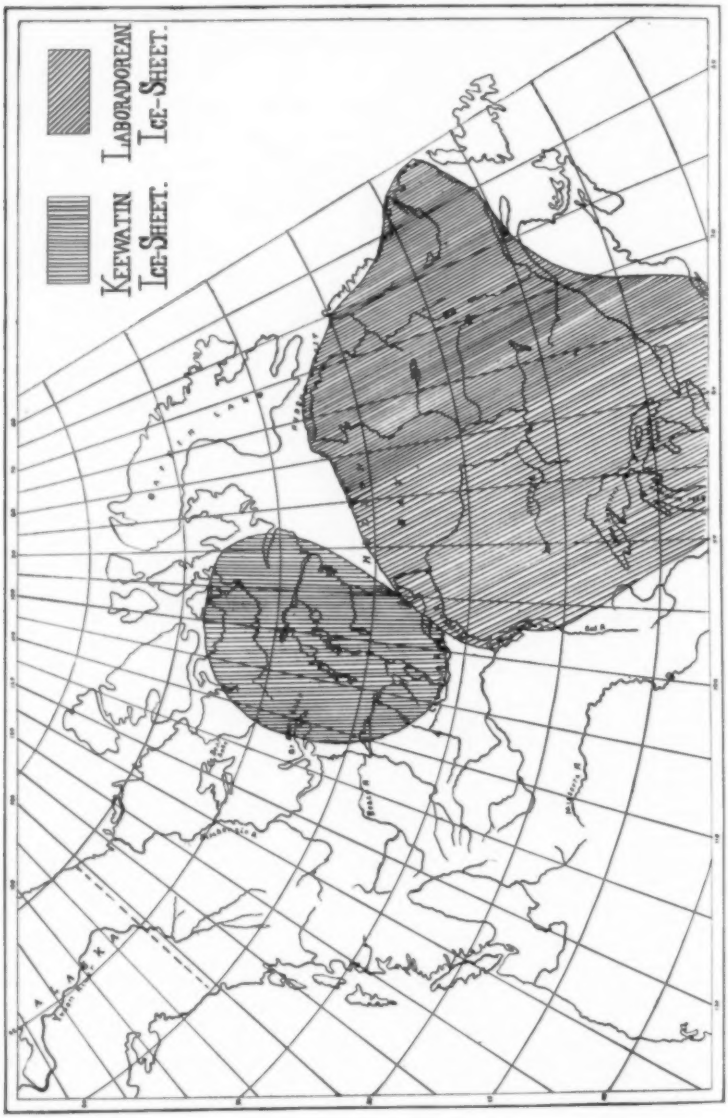
Traces of the existence of the streams that flowed eastward from the face or side of this glacier were found in several places in the form of deep potholes or giant's kettles, excavated in the summits or on the eastern slopes of knolls of granite, and gneiss where they could not have been formed under present conditions. At one place, on the south side of Berens River, several of these potholes occur on the east side of a granite knoll, one of them, at least, being ten feet in depth, and about thirty inches in diameter from top to bottom. The ten-foot hole was cleaned out, and was found to contain a great many rounded pebbles, all of Archæan rocks, some similar to the rocks of the surrounding country, and others that had evidently been transported from a distance. Both this and most of the other rocky hills where potholes were seen, have been scored and scraped down by the later glacier from the east, the outer sides of some of the holes having been cut away, leaving rounded niches in the faces of the smooth hillsides.

While a portion of the Keewatin glacier flowed southward in the Winnipeg basin, another parallel glacial stream would seem to have traveled southward between the Porcupine and Duck mountains on the east, and the rising land now marked by the Missouri Coteau on the west, both sides of this wide shallow depression being now at elevations of about 2200 feet above the sea. This glacial lobe probably extended southward into Dakota, and at its greatest extension it coalesced with the Winnipeg lobe over the summits of the Porcupine and Duck mountains, but for long periods, doubtless when the glacier was both advancing and retiring, the two lobes were more or less separated, and an extensive interlobate moraine was deposited on the summits of these hills. The Missouri Coteau is also considered to be the great moraine deposited along the west side of this lobe of the Keewatin glacier. Whether the glacier extended west of

the Missouri Coteau during this period is uncertain, but there is a strong morainic ridge extending from the Hand Hills northwestward by Sullivan Lake to the Beaver Hills, which may have been formed at this time. A high, stony, lumpy ridge about the same elevation as the Missouri Coteau, and north of and more or less parallel to the Saskatchewan River, between it and the Beaver and Athabasca rivers was doubtless formed a little later in the same glacial epoch.

Now, confining our attention to the Winnipeg lobe of the Keewatin glacier we find that after reaching its greatest extent in a southeasterly direction it gradually retired northward, and as it retired a portion of the Laurentide glacier which had accumulated in the country farther east, perhaps on the high land of the Labrador peninsula, advanced and the fronts of the two united. The Keewatin glacier had probably retired well north into Manitoba, and possibly beyond the northern confines of that province, before it was joined by the eastern glacier. After they had united the water formed by the melting of the two glaciers was ponded between their fronts and the high land to the south and west, and a large extraglacial lake was formed, which has been called by Mr. Upham Lake Agassiz.

As the Keewatin glacier retired still farther, the eastern portion of the Labradorean glacier continued to advance and obliterated most of the marks left by its predecessor, but here and there, on the harder rocks on the east side of Lake Winnipeg and farther north, distinct cross striæ were observed, where the later glacier had not rubbed out all the earlier grooves and striæ. The later glacier reached to the west side of Lake Winnipeg, or in some places a little beyond, its front assuming a roughly lobate form. Near the mouth of the Saskatchewan River the till formed by both glaciers is well shown, but between the two is a thickness of 12 feet of stratified sand and clay, showing that the Keewatin glacier had retired northward for a sufficiently long time before the advent of the Labradorean glacier to allow of the deposition of this thickness of water-lain lake-deposits.


KEEWATIN ICE-SHEET.
LABRADOREAN ICE-SHEET.

Two hundred miles still farther north, along the Grass and Burntwood rivers, the striæ of the two glaciers cross almost at right angles to each other, the one being clearly later than, and independent of, the other. A little farther east, north of Gull Lake and Nelson River, is a long narrow sandy esker, from 90 to 200 feet in height, running east and west, clearly formed by one of the streams draining the Labradorean glacier. This ridge of loose sand would certainly have been swept away, if any glacier had advanced from the north subsequent to its formation.

Before the fronts of the two great glaciers had separated the eastern one had again begun to retire, and as it retired a thickness of from 50 to 100 feet of stratified clays and silts were deposited in the bed of Lake Agassiz, chiefly north of the present basin of Lake Winnipeg, for there some large streams draining the Labradorean glacier discharged into the lake, bringing with them a heavy cargo of glacial mud. The positions of these streams are still marked by long and high eskers, which may be seen near the banks of the Nelson River, crossing the country in a direction parallel to the later striation.

The Nelson River, in its northern course, from Playgreen Lake to Split Lake, marks the approximate eastern limit of this deposit of stratified clay, and along the eastern shores of Lake Winnipeg the stratified clays were not found at a greater height than 150 feet above that lake, and, except at one place, at no great distance back from its shore.

The absence of these stratified deposits would tend to show that the eastern glacier had not retired to any considerable distance east of Nelson River and Lake Winnipeg, before Lake Agassiz was drained by the gradual shrinkage of the Keewatin glacier to a small area in the vicinity of Doobaunt and Yath-kyed lakes.

Subsequently the Keewatin glacier appears to have broken into two or more smaller glaciers, the centers of which lay still nearer the coast of Hudson Bay than the center of the single glacier. One of these centers rested over the hills southeast of Yath-kyed Lake, and from it the ice radiated in all directions,

while another probably rested over the country north of Baker Lake and Chesterfield Inlet. Even at the present day it would take but a slight reduction in temperature, or a slightly greater precipitation, to cause that northern country to be covered with snow, for in the middle of August heavy patches of snow were seen resting on many of the hillsides and Doobaunt Lake was almost completely covered with a thick sheet of ice.

After the glaciers had been greatly reduced, or had entirely disappeared, the land west of Hudson Bay stood about 500 or 600 feet below its present level, and subsequently it gradually rose until it reached its present condition of comparative stability.

Whether the three great glaciers here referred to, namely, the Cordilleran, the Keewatin and the Labradorean, originated at the same time or not I do not know, and whether they waited for one another's disappearance or not I do not know; but this much appears certain that the Cordilleran glacier had reached its greatest extent and had retired, before the Keewatin glacier reached its extreme western limit; and that the Keewatin glacier, after covering the Plains region of central Canada for a great length of time, had retired a long distance toward its central gathering ground, before the Labradorean glacier reached its utmost western limit, and that it had shrunk to a very meager representative of its former greatness, when the latter glacier was still of magnificent propositions.

J. BURR TYRRELL.

THE USE OF LOCAL NAMES IN GEOLOGY.

No field of knowledge has ever experienced in the same short period, such a rapid expansion as have the geological sciences in these closing years of the nineteenth century. With this unparalleled advancement of modern geology, occasioned by the changes in fundamental conceptions, and the application of more refined methods of investigation, there has appeared, in every branch, an endless multitude of new and often seemingly useless names. The dropping of the old and familiar terms, the change in meaning of those retained, and the introduction of an unheard-of host of others, has brought forth long and emphatic protests against such innovations.

Many of these protests are not untimely. They come not alone from the layman, but from teachers and specialists. Everyone, who has come in contact with those not specialists, knows that the vast mass of technical terms and the cumbersome verbiage that is everywhere met with in the natural sciences are most disheartening features to the student, and at once raise well-nigh unsurmountable barriers to those who would only be too glad to take up such subjects. To the adoption of an elaborate terminology in any science this phase of the subject presents an obstacle more serious than all others combined.

The whole question is one that cannot be decided by discussion, no matter how able may be the arguments presented on either side. It is not whether the old terms alone are to be used in place of the free and unlimited coinage of new ones at a ratio perhaps of 16 poor ones to 1 good one, but whether from the very nature of the attendant conditions, the adoption of either plan is feasible. It seems not. As long as any branch of science lasts the specialists in that department will continue to introduce new terms to denote new conceptions and to make definitions more precise. In spite of all that others can do,

changes in terminology will go on unabated. Protest is of no avail. A refusal on the part of the general scientific public to understand or to use the new names cannot prevent their adoption. Such action merely sets the majority outside the realm of influence. It is the specialist who sets the pace in nomenclature; others must keep up or drop out entirely. There is no other choice.

Protests against the use of new scientific names are really aimed at the unnecessary terms. In all of these protests there is almost invariably a failure to distinguish between two very different classes of terms. On the one hand the launching of new names is accompanied by a conscientious desire to better the condition of a science by clothing with suitable words the new ideas; on the other hand there is what a recent writer has aptly called a "prevalent and apparently incurable form of mania which busies itself in burdening science with a useless and formidable terminology." The first cannot be too highly commended, nor the second too deeply deplored. To be sure, it is not always possible for one not thoroughly up in a particular department to clearly discern, except in a few cases, the useful or the useless. Time alone can determine. Every progressing science must finally discard all of those titles that have served their purpose. It must also be prepared to receive all of the new ones demanded. Indeed, the rapidity with which a science is advancing is measurable, with but small degree of error, by the number of useful terms that are being proposed.

Much as it is to be deplored, it is nevertheless a fact that the mill from which the large and indifferent grist of new names is continually streaming is not wholly in the hands of those best qualified to manage it. From the very nature of the case there must ever be in its running almost no control. The real factor rendering the mighty host of unfamiliar titles so appalling is that a very large proportion of all of the new names published in the various sciences are proposed by those who are least fitted for the task. The great burden which the literature of a science must carry is the work of amateurs or those who are incapaci-

tated for real scientific work, but fancy that knowledge is advanced, or a new science is founded when they let loose a flood of odd names upon the unoffending world. The inundation is more disastrous in some other departments of science than geology, but among the geological branches it is most apparent, perhaps, in palæontology, where the majority of the names catalogued are spurious.

Fortunately for everybody concerned there is a remedy for the evil that is as divine in its effect as was the discovery of ether in alleviating the physical sufferings of mankind. There is an immutable law that determines the perpetuation of scientific terms, regardless of the quality or of the countless myriads proposed. It is the same law that governs in literature, life and all else — the survival of the fittest. It makes no difference what the new terms are, or where they apply. If they are appropriate, useful and expressive they will last — but not fixed or for all time, only until they have fulfilled their mission, until others more harmonious with the ever-changing conditions take their places. If the proposed titles fail to meet a long felt want they are at once dropped, and forever forgotten. Which, among the new terms proposed, are the really useful ones, the ones destined to survive a while, and which the unnecessary ones doomed to perish at their birth, no man can tell. Moreover, it is absolutely beyond the power of its author, or of any other person, to say which names shall be perpetuated and which shall not. Every new term depends for its life not on the wish of its originator but on its own merit. It goes for what it is worth. With the many others it takes its chances. The final tribunal is the scientific public.

In the application of technical titles neither of the two extremes, too many or too few terms, is desirable. It is not advisable, if the best interests of geological science are to be considered, to adopt by itself either a rigid, unchangeable, and spare system of nomenclature, or one in which there is ponderous verbiage. The proper mean can be reached only after .the long and fierce struggle for existence is over. The adoption of

a terminology modeled according to the first extreme, inflexible and unchanging, manifestly can never meet the wants of a growing science. The establishment of such an arbitrary system could be defensible only in the case of a dead subject—a condition that no geologist is willing to admit for his science. Much as simplicity of statement should be sought it is not always possible, nor is it always desirable, that it should be followed at the expense of precision and easy understanding.

On the other hand, a scheme built upon the plan of the second extreme is less likely to be tolerated than the first. What is gained in terseness and exactness may be wholly lost in other directions. The new-born terms become mere symbols, perfectly meaningless and useless to all except the author. There is great danger of producing exactly the opposite effect from that intended. Instead of a beautiful fabric, the wonder and admiration of all, there is merely a lifeless, shapeless mass, shunned by many, cared for by none.

There is another phase of the question about which exists much confusion. Little mention is ever made of the twofold field of usefulness for which every science is designed; and in none is a dual conception more important than in geology. The one phase requires a terminology that is technical and special, that is established primarily for the active investigator and that is in no way intended to be memorized by the layman or any others, or to reach outside of a small and select circle. There will always exist a need for some such terms and the terms will always come in reponse to the need. No amount of protest will frighten the specialist out of using them. It may clutter up the scientific literature; it may be the bugbear of workers in closely allied branches of science; it may divert the attention of many from the subject itself. No matter. It has come to stay.

The other field demands names that are general, popular, simple, and free from technical appearance. The literature thus established is intended for an entirely different class from that which the first category takes into consideration. This distinction is rarely made, yet no one can doubt the existence of

two wholly different audiences. In the presentation of every discourse the latter cannot be for both. It must be one or the other. It is the inalienable right of every author and every lecturer to select his audience. No one wishing to reach the many would think for a moment, of letting loose on his listeners or readers a flood of absurd and meaningless technicalities ; nor would a small, selected group of specialists wade through unknown depths of "simple" verbiage.

The coining of new terms to designate new ideas or more precise definitions has a bearing still broader than any yet considered in any of the numerous protests that have been presented. The layman complains of the host of "long names" with which every branch of science abounds ; the scientist criticises the terminology employed in the various branches other than his own ; the specialist bemoans the deplorable condition of the nomenclature in all branches as well as his own. Now, so far as the question under consideration is concerned, all are on identically the same plane. When a reason for this is sought only one stands out permanently. Each critic is, in reality knocking loudly for admission to other departments, without the same hard work and training that he has bestowed upon his own. Moreover, the protests against the established terminology are all one way ; were they not, the opposite view would not be so totally obscured. The demands for transfer from one department to another is invariably from the more simple and general to the more complex and special. Why the layman should desire to leave his own field to enter the domains of science unknown to him is as inexplicable as why a stratigraphical geologist should want to become a geographer or petrographer, or *vice versa*. Seldom does a scientist think of becoming an artisan. Yet if he should desire to do so, he would be, after five minutes' talk with a machinist, carpenter, or electrician, confronted by so many unfamiliar terms — technical terms of every day use — that he would at once cry out for greater simplicity of language. In the rapidly advancing branch of applied electrical science, for example, new terms are constantly appearing. The reason that

these technical names are so difficult to understand is that each is an epitomized history of the special part, its position and function in the complete mechanism.

In the geological sciences the technicalities play the same rôle as they do in the arts and in business. To the large majority of people the name Monadnock, for instance, may mean only a big building, a war ship, or an Indian, but to the professional geographer it has attached to it a special meaning In a single word it sums up the complete life history of a particular kind of relief feature — a history that would require the space of a long chapter to describe in "simple" language every time it is referred to. If such a term chance to be a happy choice, if it save the busy worker the writing of several pages in order to express the same idea in another way, or "if it prove to be acceptable to workers in its field," as its author says, "it will take root and flourish; if not, it will soon wither away and be seen no more."

Granite, trap and greenstone, may be good enough "simple" names to apply to certain rocks, but the terms have become so general that in exact work they now mean almost nothing. To the petrographer the name pegmatite at once suggests a variety of granite that has a long and intricate history, totally different from dozens of other kinds of granite, each having a record equally complicated and equally diverse. This term incorporates in three short syllables history enough to fill a large volume. But he who wishes to know something about this particular kind of granite called pegmatite, little cares to waste his time in going over the whole literature of granite that he may get a little of the desired information. Likewise, who will not say that such a name as websterite, applied to a dark colored trap-like rock, does not at once separate the mass from a hundred other stony aggregates having a similar general appearance, and at the same time indicate a whole train of important events that would be otherwise passed over if the more popular name of trap were used alone. It may be argued that these are useful and necessary new names. Yet who could pass judgment on

them until after they were proposed? And who can pass judgment on any terms until they are suggested? Or who can say whether they would be useful or not until they are tried?

In this day and age the geological sciences are protected at the start by one great safeguard against the promiscuous introduction of new names. There is one test that every new name must stand before it can venture to ask for recognition. This is the test of definition. Every new name in geology must be properly defined before it can be noticed at all. Its subsequent career depends upon its utility.

It has been already intimated that the rate with which a science is advancing is measurable by the number of new and useful terms that appear. At present this statement is especially true of some of the geological sciences. To be sure, new terminology does not necessarily indicate new facts, but when new terms receive the favor of those best qualified to pass judgment upon them, of the specialists in the particular department, when also these names stand for new conceptions, the branch of knowledge thus affected is certainly undergoing such radical change that the final outcome must be essentially very different from the old. This rapid change in terminology is at present characteristic of several of the branches of geology.

In no department has the coining of new names gone on more vigorously than in stratigraphical geology. The reason is to be found partly in the inherent conditions existing in this field, and partly in the complete change of base that this branch of the science has undergone in late years. The fundamental conception of the geological formation, whether large or small, whether a single bed or a great series, is a sharply defined "geological unit," instead of a vaguely bounded "group" of layers. The former is now capable of being clearly demarcated by strictly physical characters, that are the direct outgrowths of the actual conditions giving rise to the formations themselves; the latter is too often based upon trivial or accidental characters which are

relatively unimportant as critical criteria, either in classification or correlation.

The principle underlying the recent naming of geological formations gives to each stratigraphical unit a special geographic designation, derived from some prominent town, watercourse or land form, within the boundaries of that formation. As thus established the latter is a well-defined and independent unit, having a definite place in the general geological scheme, no matter how this may change afterward, or what method of classification may be followed. This definite stratigraphical unit contrasts strangely with the unwieldy, ill-defined and usually little understood large formation of the past, the very name of which indicated either lack of exact knowledge of it itself, or a covering up of ignorance regarding its affinities. By this new method, or if it be more exact, by the vigorous application of an old principle that was so loosely followed as to be almost unrecognizable, geological nomenclature has been certainly greatly increased, even enormously enlarged by the introduction of the new plan. The former list of names numbered only about two score, indicating all of the smallest subdivisions which went to make up the general geological column. The names of the new list run up into the hundreds and even thousands, are different in every considerable area, and additions are constantly being made.

It is against this copious multiplication of geological names that the protests have been chiefly made. Curiously enough the struggle has been reduced to a clash between the practical field geologist on the one hand, and on the other the laboratory worker, those especially interested in the other departments than stratigraphy and the palæontologists, who see their standard classification abandoned, and their usefulness in the domains of geology diminished. And the former have won.

When, a decade and a half ago, various geological surveys in this country were established or reorganized, those entrusted with the work soon found that, if speedy and exact results were to be secured, and if substantial data were to be obtained upon which all other workers could also build, something else must

be devised than the existing scheme of vaguely defined geological formations, having no comparable limits in different provinces and even diverse values in different parts of the same province. A natural, yet elastic foundation must be secured. Practical experience and the demands of the times quickly pointed out a suitable plan. So well has it served the purpose and so readily adaptable is it to the varying conditions met with on all sides, and the unforeseen exigencies constantly arising, that it has brought under its standard nearly every practical field geologist.

The method of designating geological formations by geographical names certainly does greatly increase the nomenclature, at times seemingly to a burdensome extent. This appears to be the only objection that has been yet urged against it that might call for notice. Yet, to all, except those who do not care to go beyond the ordinary text-book in geological work, even this seems hardly necessary, since it is offset by so many manifest advantages.

It may be truly said that no greater boon to the working geologist has been yet devised, than the plan of designating, geographically, geological units irrespective of exact position or age. Incorporated in the new plan are all the salient good features of the old method, while none of the objectional ones are retained. Since its adoption a vast mass of valuable information has been obtained that was previously unthought of, information that is in a shape to be always used, without the necessity of the user personally going all over the ground again; the other departments of geology have been greatly aided; and stratigraphical geology itself has made greater real progress in the short decade that has elapsed since the method with its new impetus came into general use than in all time previous. In the same short period more has been learned about the nature of sedimentation, the actual relations existing between rock formations, and the structure of the lithosphere than was possible before. In fact a rational physical basis for geological correlation and classification has been found.

The real meaning, then, of the multitude of new names that has recently made its appearance in the literature of stratigraphy, is the practical adoption of more refined methods of geological work, the provision of suitable means for the collection of more exact geological data, and the grasping of more advanced and rational conceptions regarding geological correlation and classification.

CHARLES R. KEYES.

THE WEATHERED ZONE (SANGAMON) BETWEEN THE IOWAN LOESS AND ILLINOIAN TILL SHEET.

PRELIMINARY STATEMENT.

Extent of Illinoian till sheet.—The Illinoian till sheet here discussed was formed by the Illinois glacial lobe in connection with the maximum extension of that lobe. It seems quite well established that a lobe on the east, which covered southeastern Indiana and southwestern Ohio and extended a short distance into Kentucky also had its culmination at the Illinonian stage of glaciation. Farther east the Wisconsin sheet in many places reaches the glacial boundary, but there are small tracts of drift older than the Wisconsin, lying outside its limits in eastern Ohio, northwestern and northeastern Pennsylvania, and northern New Jersey, which may prove to be of Illinoian age, though this is as yet not established. To the west of the Illinois glacial lobe there is a large area covering northern Missouri, southern Iowa, northeastern Kansas and eastern Nebraska, in which the upper sheet of till is older than the Illinoian, and is now referred to the Kansan stage of glaciation. The lobe which formed it is here referred to as the western lobe, for it has as yet received no more definite name. The Illinoian sheet has not been recognized farther west than the limits of the Illinois glacial lobe. It seems probable, however, that it may be found in the western region, and possibly it occurs as far south as northern Iowa.

The Illinois glacial lobe at its maximum extension to the southwest, crossed the Mississippi and encroached a few miles on Iowa, in the district between Clinton and Ft. Madison. But farther north and south it appears to have terminated east of the Mississippi, except, perhaps, for a few miles near St. Louis, Mo. The southern border of this lobe apparently reached to the glacial boundary from St. Louis eastward as indicated above. It is

the southwestern border which claims our attention at this time, since the Illinois lobe there overrode to some extent the sheet of Kansan drift, formed by the western lobe which covered much of Iowa and portions of the neighboring states.

The southwestern limits of the Illinoian drift is usually marked by a definite marginal ridge or by chains of knolly and slightly ridged drift. Beginning at the south, in Jersey county, Ill., a few miles north of St. Louis, and tracing northward, the margin is found to follow the east side of the Illinois River in Jersey and Greene counties, and to carry only occasional knolls and low ridges. It crosses the Illinois in southeastern Pike county and takes a northwest course, coming to the Mississippi bluff near the line of Pike and Adams counties. It there enters a district which had been covered by the western lobe at the Kansan invasion. The Illinoian border takes a northward course along or near the east bluff of the Mississippi through Adams and Hancock counties. A definite ridge twenty to forty feet high is developed along much of the Illinoian margin in Pike and Adams counties, and as far north in Hancock county as a point opposite Keokuk, Iowa. For a few miles above Keokuk the Mississippi River apparently follows nearly the border of the Illinoian till sheet, and no definite ridges are found. At the bend of the Mississippi below Ft. Madison the Illinoian border crosses into Iowa. Its marginal ridge can be traced without difficulty from the vicinity of the Mississippi River bluff south of West Point, Iowa, northward through Lee, southeastern Henry, northwestern Des Moines and western Louisa counties to the Iowa River at Columbus Junction. Its course there changes to the northeast, and it can be traced diagonally across Muscatine county from its southwest to its northeast corner. It has been traced no farther to the northeast because of concealment by a heavy sheet of loess which borders the Iowan till in Scott county, Iowa. It is known to extend as far north as Scott county, for the Illinois till sheet has been observed in southern Scott county as far east as Davenport. The concealment by the Iowan loess is very great, not only in Scott county, Iowa, but also in Rock Island,

Whiteside and Carroll counties, Ill. It becomes a difficult matter, therefore, to decide upon the position of the margin of the Illinoian drift in any of these counties. It is also not fully decided whether it reaches to the border of the driftless area in Jo Daviess and northwestern Carroll counties, Ill , and in southwestern Wisconsin. The balance of probabilities, however, seems to favor its extending to the driftless area.

The Illinoian till sheet overlaps a few miles the Kansan till sheet of the western lobe from the latitude of Hannibal, Mo., northward to the vicinity of the southern point of the driftless area. In this region of overlap a weathered zone is developed between the Illinoian and Kansan till sheets at the level of the outlying Kansan surface as indicated below.

Introduction of the name Illinoian.—The tracing of this southwestern border of the Illinois lobe was begun by the writer in the autumn of 1892, and carried as far north as Hancock county, Ill., that season. No opportunity to continue the study was afforded until the spring of 1894, when the mapping of the border was carried from Lee county, Iowa, northward to Scott county. The greater part of the data presented in this paper, and conclusive evidence of a long interval between the deposition of the till sheets now known as the Kansan and Illinoian, and also the evidence that the Illinoian is much older than the Iowan had been obtained as early as June 1894. The writer then began to use the name Illinoian in correspondence, but it seemed best to defer its introduction into literature until opportunity had been afforded other geologists to examine it. In August 1896 Professor T. C. Chamberlin and Dr. H. F. Bain were conducted by the writer to some of the exposures in southeastern Iowa which show the soil above and below the sheet formed by the Illinois lobe, and each recognized the need for a distinctive name for this drift sheet. The name was accordingly soon introduced into geological literature by Professor Chamberlin.[1]

Other interpretations.—At the ninth annual meeting of the Iowa

[1] See editorial JOUR. GEOL., October–November 1896, pp. 872–876.

Academy, held December 1894, Mr. F. M. Fultz read a paper[1] in which the interpretation was presented that the ice lobes alternated in the occupancy of the district south of the driftless area, and that the latest occupancy was by the western lobe. The extension of the eastern lobe into Iowa had been inferred by him through the discovery of a bowlder of red jasper conglomerate near Augusta, Iowa, which was apparently brought from north of Lake Huron. The evidence of an extension of the western lobe over the same district was found in eastward-bearing striæ along the brow of the Mississippi bluff at points farther east than the site of this bowlder. Mr. Fultz argued that if the striæ are not the product of the latest invasion they would not have been preserved in such an exposed situation. He also referred to some bowlder strewn terraces in the Mississippi valley at and above Keokuk as moraines, and correlated them with the striæ as the product of the last ice invasion. The following summer Mr. Fultz and the writer, while examining some rock outcrops in Burlington, found a striated surface in which the bearing is westward. This was evidently produced by the Illinois lobe, and as it is in a section about as exposed to obliteration by a subsequent invasion as those cited by Mr. Fultz in his paper it became necessary to readjust the views set forth in that paper. This was done at the tenth meeting of the Academy in December 1895, and the question of the relation of the two invasions was there left somewhat in doubt.[2] The bowldery terrace interpreted by Mr. Fultz to be a terminal moraine has been examined by Professor T. C. Chamberlin and Dr. H. F. Bain, as well as by myself, and to each of us it seems best explained as a residue of coarse material formed by a stream excavation along the Mississippi valley subsequent to the later ice invasion. The evidence that the Illinois lobe was last on this ground seems conclusively shown in the relation of its till sheet to that of the sheet formed by the western lobe. The latter can be traced under the Illinoian sheet as indicated below. In addition

[1] Proc. Iowa Acad. of Sciences, Vol. II, 1895, pp. 209–212.

[2] *Ibid.*, Vol. III, 1896, pp. 60–62.

to this evidence there is found an abandoned river channel in the district immediately west of the limits of the Illinoian drift which carried southward the drainage outside the Illinois ice lobe. The banks of this channel are well defined, and the channel evidently has not been filled by the drift of any subsequent invasion.

Extent of the Iowan loess.—By the term Iowan loess is meant that sheet of loess which connects at the north with the Iowan till sheet. A till sheet of Iowan age has been found in northern Illinois as well as in eastern Iowa and it probably covers the greater part of the northern half of Illinois. It is, however, covered by the Wisconsin till sheet from Bureau county, Illinois, east and south. How much of Indiana and Ohio was covered by the Iowan ice invasion has not been determined. The Iowan till certainly does not extend as far south as the Wisconsin in those states. The loess forms a heavy deposit along the border of the Mississippi and Illinois valleys, but is comparatively thin in the region east of the Illinois, its average thickness being scarcely ten feet. A silt tentatively correlated with the loess covers the Illinoian till sheet wherever exposed outside the Wisconsin from the Illinois River eastward to central Ohio. The Sangamon weathered zone between the loess and the Illinoian till sheet is found from central Ohio westward to southeastern Iowa, *i. e.*, to the limits of the Illinoian till sheet. The Iowan loess extends also over the Kansan till sheet of southern Iowa and adjacent portions of Missouri, Kansas and Nebraska, but this loess is separated from the underlying till by a much longer interval than that between the loess and the Illinoian till sheet, an interval comprising two interglacial stages and one glacial stage.

Application of Buchanan.—At the tenth annual meeting of the Iowa Academy Professor Samuel Calvin, after describing certain gravel deposits in northeastern Iowa, introduced the term Buchanan as a name for an interglacial stage following the Kansan,[1] and made the following statement concerning the origin and age of the deposits:

[1] Proc. Iowa Acad. of Sciences, Vol. III, 1896, pp. 56-60.

As to their origin the Buchanan gravels are made up of materials derived from the Kansan drift. As to age they must have been laid down in a body of water immediately behind the retreating edge of the Kansan ice.

Manifestly the deposition of the Buchanan gravels covers but a small part of the time between the Kansan retreat and the Iowan advance. Unless therefore the deposition and subsequent weathering both be included under this name it does not fill an interglacial stage. Were there no Illinoian glacial stage to break the continuity of interglacial conditions from the Kansan to the Iowan stage of glaciation it would not seem necessary to look for other terms. But in view of this glacial interruption there seems need for names which will stand for the weathered zones above and below the Illinoian till sheet. It is for this reason that the name Sangamon is here suggested for a weathered zone separating the Illinoian till from the overlying loess. In an accompanying paper the name Yarmouth is introduced for the weathered zone between the Illinoian and Kansan till sheets. The name Buchanan may still have the significance given it by Professor Calvin; and if weathering be included may perhaps be used to cover the time involved in the two interglacial stages with the intervening glacial stage.

THE SANGAMON WEATHERED ZONE.

Earliest recognition.—Apparently the first recognition of the occurrence of a definite soil horizon between the Iowan loess and the Illinoian till sheet is that reported by Professor A. H. Worthen, in the Geology of Illinois.[1] In his report on Sangamon county, Illinois, made in 1873, Professor Worthen called attention to a soil found at the base of the loess in Sangamon and neighboring counties. The soil apparently was first noted by Mr. Joseph Mitchell, in the excavation of wells in the northwest part of the county and in neighboring portions of Menard county. Mr. Mitchell furnished for publication in the Geology of Illinois the following section of the beds usually penetrated.

[1] Geol. of Illinois, Vol. V, 1873, pp. 306-319.

	Feet
Soil,	1 to 2½
Yellow clay,	3
Whitish jointed clay with shells,	5 to 8
Black muck with fragments of wood,	3 to 8
Bluish colored bowlder clay,	8 to 10
Gray hardpan — very hard,	2
Soft blue clay without bowlders,	20 to 40

Professor Worthen states that the bed overlying the black muck is undoubtedly loess, also that the black muck indicates conditions suitable for the growth of arboreal vegetation in the interval between the deposition of the bowlder clay and the overlying loess. The name Sangamon is taken from this locality where the soil was first reported.

General prevalence of a weathered zone at the base of the Iowan loess.—In the locality just mentioned there appears to be only a bed of muck to indicate the interval between the deposition of the bowlder clay and that of the overlying loess, for the clay immediately below the muck is described as of a blue color, a feature which suggests that there was not much oxidation and leaching, or else that there was subsequent deoxidization. The more common phase is a reddish brown till surface for which Dr. H. F. Bain has proposed the Italian name "ferretto,"[1] which may or may not be accompanied by a black soil. This reddish-brown surface appears to have been developed in all places where there was fairly good drainage. But in places where the drainage was imperfect a black muck of considerable depth accumulated and the reddened zone was imperfectly or not at all developed. In western Illinois the exposures of a black soil at the base of the loess are relatively few, but the reddened till surface is a common feature in every township. In much of the white clay district of southern Illinois and in portions of the Sangamon drainage basin a black soil is well developed. It is also well developed in southeastern Iowa. Where the black soil is best developed leaching is found to have extended in places only one to two feet into the underlying till but it often extends to a depth of

[1] See Proc. Iowa Acad. of Sciences, Vol. V, 1898 (in press).

six feet or more. Where it is absent the leaching generally extends to a depth of six feet below the base of the loess. The variations in depth of leaching appear to depend on the conditions for percolation of water, being greatest where percolation is most rapid.

Noteworthy exposures of Sangamon soil.— A few instances of the exposures of this soil are selected which will illustrate the variability in its character. The first section, at Ashland, Ill., is near the place where Professor Worthen reported its occurrence.

The following series of drift beds were penetrated by a coal shaft at Ashland, the identifications being made by the writer from samples of the material preserved at the engine house:

	Feet
Soil,	1½
Yellow loess, fossiliferous,	9
Blue loess,	2
Peat with black, sandy slush,	22
Bluish, gummy clay with few pebbles,	20
Yellow till,	30
Total drift,	84½

At the air shaft sand was found in the place of the blue gummy clay beneath the peaty slush. A similar thick bed of peat has been noted at several other points in that region, one of the most conspicuous being in a well at Virginia City made by Mr. Oldridge. The peat was entered at the base of the loess at about 15 feet and continued to a depth of 28 feet, beneath which a blue gummy clay was found. The drift at Virginia City has a depth of 187 feet, as shown by the coal shaft. This shaft is reported to have passed through a lower black soil between till sheets at 67 to 70 feet.

In the south part of the Sangamon basin, in the vicinity of Taylorville, Ill., the loess which has a thickness of 10 to 15 feet is underlaid by beds of sand and gravel, carrying thin peat beds in their midst as well as at the junction of the loess and the sand. At the Taylorville coal shaft the uppermost peat bed was found at 13 to 15 feet, and the lowest at 40 to 44 feet. Numer-

ous exposures of this peaty material alternating with sand beds may be seen in ravines in that vicinity.

In October 1896, Professor Chamberlin and the writer examined together numerous exposures of the Sangamon soil in the portion of eastern Illinois south of the limits of the Wisconsin drift, chiefly in Cumberland, Coles, and Shelby counties. North of Greenup there are exposures where the subsoil beneath the Sangamon soil is traversed by branching rootlike tubes one to two inches in diameter, which were easily traced ten to twelve inches below the soil proper. These tubes are filled with the black soil which apparently settled into them upon the decay of tree roots. There seems to us little question that the Sangamon soil here supported a forest. The till below this soil in these counties shows leaching to a depth of several feet. It also presents weathered cracks and seams extending down a depth of 20 feet or more. Similar leaching and weathering below the Sangamon soil has been observed by the writer in several other counties in southeastern Illinois, and in Vigo, Clay, and Sullivan counties in southwestern Indiana, thus extending it to the southeast border of the Illinois lobe.

Returning to western Illinois, excellent exposures of black soil and leached subsoil are found along the Santa Fé railway in eastern Knox county. The soil may be seen distinctly at a distance of nearly one-fourth mile. It is of a deep black color, resembling the surface muck found in flat portions of the uplands. The till beneath it has been leached to a depth of about four feet. The loess has a thickness of 12 feet, and is slightly calcareous in the lower portion. The entire leaching of the till may confidently be referred to a date earlier than the loess deposition.

At Galva, Ill., a black soil at the base of the loess is well exposed in a clay pit at the brick yards east of the city. A large log was found imbedded in this soil, which here has a depth of two feet. The overlying loess is 15 feet in depth. A well at the brick yards penetrated 40 feet of till below the buried soil, of which the upper 30 feet has a yellow color and the remainder a blue-gray color.

In southwestern Carroll county, Illinois, there are extensive exposures of a soil at the base of the loess, made by the Chicago, Burlington and Northern Railway Company, the loess having been removed to make a fill across the valley of Johnson Creek. Probably a half acre of the buried soil is here exposed to view. It has a deep black color to a depth of 10 or 12 inches, beneath which it assumes a greenish yellow color, such as is presented by subsoils beneath poorly drained regions. This subsoil is leached as far down as exposed, a depth of three feet. This locality was visited last November by Professors Calvin, Udden, Bain, and myself, and each recognized the clear indications of a long interval prior to the loess deposition. It may be noted in this connection that Judge James Shaw mentioned a soil in Carroll county in his report on the Geology of Illinois, which apparently has the same horizon as the one just described. It was found at a depth of 15 feet and a deposit of wood two or three feet in thickness was associated with it.[1]

On the portion of the Illinoian sheet in southeastern Iowa many excellent exposures of the Sangamon soil are found. An exposure similar to that in Carroll county, Illinois, has been made at West Point, Iowa, where the Chicago, Ft. Madison and Des Moines Railway Company has excavated to obtain filling for its tracks. The loess has been removed over an area several rods square, leaving the buried soil at the base of the excavation. Although the exposure is on the crest of the ridge which marks the western limits of the Illinoian drift, the soil is of a deep black color and has a depth of several inches. This exposure was visited by Professor Chamberlin, Dr. Bain, and myself in August 1896, as were also several roadside exposures between West Point and Denmark, and between Denmark and Ft. Madison.

Exposures in other portions of southeastern Iowa are given in connection with the discussion of the Yarmouth weathered zone.

Valley excavation during the Sangamon interglacial stage.—The large streams in western Illinois and southeastern Iowa are

[1] Geology of Illinois, Vol. V, p. 80.

characterized by high-level terraces. The valleys of which these terraces are the bottoms have been formed in the Illinoian till sheet, and are covered by the Iowan loess. The excavation may, therefore, be referred to the Sangamon interglacial stage. They are broad and very shallow. On Skunk River, along the borders of Lee and Des Moines counties, Iowa, the terrace is only 30 to 40 feet below the level of the uplands, but the valley is nearly two miles in average breadth. The valley cut below the level of the terrace is more than 100 feet in depth, but is only one-half mile in average breadth. These features indicate that during the Sangamon interglacial stage the stream had a lower gradient than at subsequent stages. On the neighboring portion of the Mississippi the valley formed at the Sangamon stage was shallow, as on Skunk River, but was not much wider than the inner valley. The large volume of water flowing through the valley at the time when it constituted an outlet for the glacial Lake Agassiz and the glacial lake in the Superior basin is perhaps the cause for the relatively great erosion subsequent to the Sangamon interglacial stage.

In southern Illinois and southwestern Indiana the main streams usually flow in broad shallow valleys, in some cases several miles in width, which were apparently built up by the glacial and fluvio-glacial deposits of Illinoian age. It is seldom that sufficient deepening of streams has occurred to produce well-defined terraces; and it is not an easy matter to determine the amount of work accomplished during the Sangamon interglacial stage. On the borders of these lowlands the Iowan loess rises above the level of the modern streams and at such places occasional exposures were found in which the junction of Iowan loess and the Illinoian till is marked by a thin bed of material more pebbly than the typical till; a feature which is thought to indicate moderate stream action prior to the deposition of the loess. A similar feature has been noted on the borders of many of the small valleys in western Illinois and southeastern Iowa.

FRANK LEVERETT.

STUDIES IN THE DRIFTLESS REGION OF WISCONSIN II.[1]

SINCE my article which appeared in the November-December number of the JOURNAL OF GEOLOGY was written, much additional evidence has accumulated, largely along new and supplementary lines. There have, however, been some additions along the lines there developed, which I beg to notice in an extended footnote.[2]

[1] On page 834 of my last article a change in the paragraphing somewhat obscured the course of the reasoning. The objections to torrential action were grouped under three heads: *a*, transverse ridges, beginning at the tenth line from the top; *b*, the lateral ridges; *c*, the size of the material.

The first head was improperly made to begin at the twenty-third line from the top, where there is only a reference to the ridge, *b*, Fig. 1. I think that I was myself partly the occasion of the mistake, since the *b* stood alone, Fig. 1 having been omitted. The third head should have been worded more in harmony with the others and more indicative of its own character.

[2] Regarding the ridge *d* (Fig. 1 of last article) I stated that it seems to belong structurally to both valleys. But the heavy masses of ferruginous sandstone which form so conspicuous a component of the ridge appear to be peculiar to the east valley. The knob *d* (Fig. 1) is composed of it. It also occurs on the north rim at *c* (Fig. 7). Although much harder than the sandstone in the same horizon on either side, it is not as prominent in the topography as we should have expected, owing probably to the fact that it has pronounced joint structure and the separate masses are rather easily dislodged. The supposed bowlder bed on the west side of the west valley (shown in gully) is of small material, undoubtedly a water deposit, leaving the ridge *b* (Fig. 1) as the terminal deposit for that valley.

An interesting feature has developed in connection with the ridge shown in Sec. 4, Fig. 2 of last article, occurring in the third valley described (position shown at *c*, Fig. 4). A well dug just in front of the line of the section struck at once into a clay resembling the loess and entirely free from stones. It continued in this for its entire depth, about twenty feet.

The terminal arrangements of the material in the last valley described (partly shown in Sec. 6, Fig. 2) displays a certain feature which claims further notice; an independence of the minor features of the topography, shown in the direction of movement and the disposition of the beds. In the accompanying figure I have represented by contour lines the original rock surface and by dots the contour of the lower end of the beds. The axis of the old valley runs very close under the eastern hill, while on the west a broad shelf rises gradually toward the nearest bluff.

Distribution of transported material on the higher slopes.— Most of the deposits previously described fall between the horizon of the present river-level and that of the highest terrace, so that although there seems to be excellent reason on other grounds

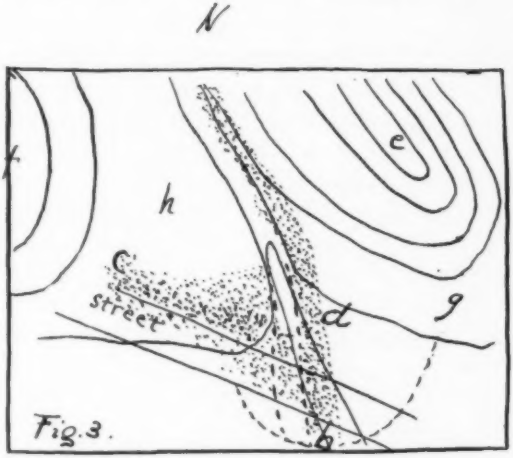


FIG. 3. Scale 300′ to an inch. Adapted from the village plat, which I used as the foundation.[1]

f. End of a short spur or projecting angle of one of the higher bluffs.

e. End of a long spur forming the northeast valley rim.

g, *h*. Rock shelves sloping gently from their adjacent hills.

ab. The bowlder ridge, the south end of which lies over the old rock valley, shown in entire lines, while the present drainage indicated by dotted lines runs further west just inside of the ridge. It has both an outward and an inward slope plainly apparent in spite of the loess, which, however, as the street cutting shows, greatly exaggerates its apparent breadth and diminishes its apparent height. The bowlder bed doubtless covers all of the shelf *h*, but being heavily covered with loess I have only indicated that part which is uncovered. On its south front it declines from a thickness of about twelve feet or fifteen feet to nothing in the width of a street.

The asymmetry of the deposits, *i. e.*, their presence on one side of a valley, and apparent absence on the other has been something of a difficulty in working out the

[1] These maps have necessarily been constructed without the aid of special measurements. But while they must needs lack the exactness which such a method would have given, they have been carefully constructed from fairly correct data, and after thorough study of the topography, and are essentially correct. The valley bottoms, however, offer especial difficulties, since the series of deposits of which the loess is the top, has been eroded so as to form a most intricate system of ravines of which only the principal ones could be represented.

for disputing the competency of running water, landslides or creep to give rise to such deposits, it cannot be denied that they lie within the horizon where such agents are operative. It is, therefore, very desirable to trace the deposits into higher levels. But the middle portions in all the valleys are so deeply covered by the loess — which has a strong tendency to fill up depressions and obliterate minor irregularities of surface — that nothing can be seen save in the rare cases in which gullies are deep enough to slightly expose the structure, and even these, although affording valuable evidence as to the sequence of the deposits, etc., give no decisive indication as to their character.

On the outlying secondary hills the loess is not so thick, and a systematic study of these has given unexpectedly interesting results. The accumulations there, unlike those of lower levels in which all the different local formations are represented, are composed almost exclusively of limestone from the tops of the

glacial hypothesis. But as shown above, while on one side the deposit may rest on a shelf, on the other it may lie in the axis of a valley, where if circumstances are favorable it may be covered by late deposits. This asymmetry is, however, quite as serious an objection to torrential action or to the flow of semiliquid material, neither of which could form deposits at a notably higher level on one side of a valley than on the other. At *c* the bowlder bed rises into a sharp ridge three or four feet high as shown in Sec 6, Fig. 2 (previous article). From *c* to *d* there is a nearly uniform eastward slope, broken only by the present drainage channel. Had there been nothing in the configuration of the ridge to confine the drainage it must have left the bowlder bed near *d*, to enter the lower level extending southeastward from thence. The dotted half circle drawn through points of approximately equal elevation on either side of the old drainage channel will show how widely the bowlder deposit departs from the normal plane of a water borne deposit.

There is an implied suggestion in Professor Chamberlin's note prefacing my first article, which requires more specific notice than I have yet given it. It is whether these deposits might not have resulted from landslides or from the lavalike flow of saturated earth during thaws, or the more gradual creep due to repeated thawing and freezing. Perhaps the best answer will be to state the conditions presented by a single case. The deposit shown in Sec. 3 (Fig. 2) of last article lies on a rock shelf and reaches to about forty feet above low water of the Mississippi, a height which is not reached in the valley for several hundred feet back. A 2° slope would not intersect the bottom of the valley at a distance much, if any, short of 1500 feet. To reach its present position from any possible source the material would have had to travel about 3000 feet and make a sharp bend in its course. Its probable source was one of the bluffs shown in Figs. 4 and 5, present article. Limestone is abundant in the deposit, and some of the fragments quite large.

higher bluffs. To this is added a comparatively small percentage of material from the transition beds at the base of the limestone.

Circs.— Short, direct valleys, with broad heads and narrow outlets, are finely shown in this vicinity. The deposits found in

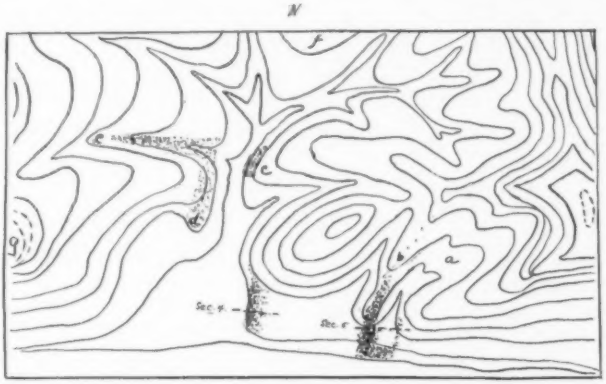


FIG. 4. Contour sketch map in which the center is occupied by the lower end of a large valley— the third in the series described. At the extreme right is a bluff, the highest and most massive in the group. Descending from this is the small valley or circ, *a*, the fourth described. The positions of Sects. 4 and 5 (Fig. 2) of the previous article are indicated. The bowlder deposits shown in Sec. 5 continue along up the crest of the ridge *b*, as shown by dots. I have endeavored by the greater or less concentration of the dots to indicate the relative abundance of the bowlder deposits. Sec. 4 (Fig. 2) was obtained at the point *c*. The peculiar point *d* appears to be largely or wholly composed of similar deposits. In the ravine, *e*, extensive washouts reveal very thick bowlder deposits. Doubtless similar deposits occur in the other ravines within the secondary hills, but the conditions are not favorable for observation. The lower end of the bowlder covered spur shown in Fig. 4 is seen at *f*. West of the middle of the valley, no hills reach the limestone horizon, save that at *g*. Scale 1000′ to an inch. Contour lines at intervals of 50′.

In this and the following maps I have indicated the base of the limestone horizon by heavier contour lines.

connection with them show several marked variations in detail from those found in the larger valleys, while they have a close resemblance among themselves. They all front on the Mississippi and are the result of the comparatively rapid erosion which its presence induces. The secondary hills which separate neigh-

boring circs are very wide at the outer margins, but narrow to ridges of only a few feet in width at the points where they join the primary bluffs. In some cases these connecting ridges have been so

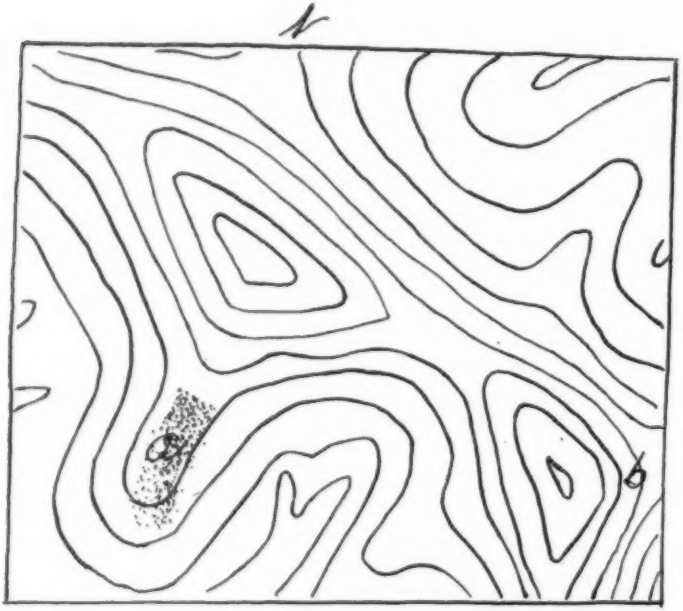


FIG. 5.—Sketch map of a portion of the same valley as shown in Fig. 4, joining that on the north—(east end). It shows a spur extending out from one of the angles of a limestone-covered bluff and having its top covered with limestone débris. It appears to extend downward somewhat on the east side, but, on account of the increasing thickness of the loess, it cannot be told how far. At *b* a spur of very similar height and form holds almost identical relations to its primary bluff, but it is entirely destitute of limestone débris. It is evident, therefore, that one has been subject to the action of some agent which did not effect the other. Scale 500′ to an inch. Contour lines at intervals of 50′.

worn away as to form a considerable sag. For convenience, I will speak of these secondary hills between circs as buttresses.

For the purpose of illustration I have selected one of the finest of these, which is shown in contour lines in the accom-

panying sketch map, Fig. 6, together with portions of the circs adjoining on either side. A portion of the main bluff, with its limestone cap, is shown in the upper right-hand corner (in order

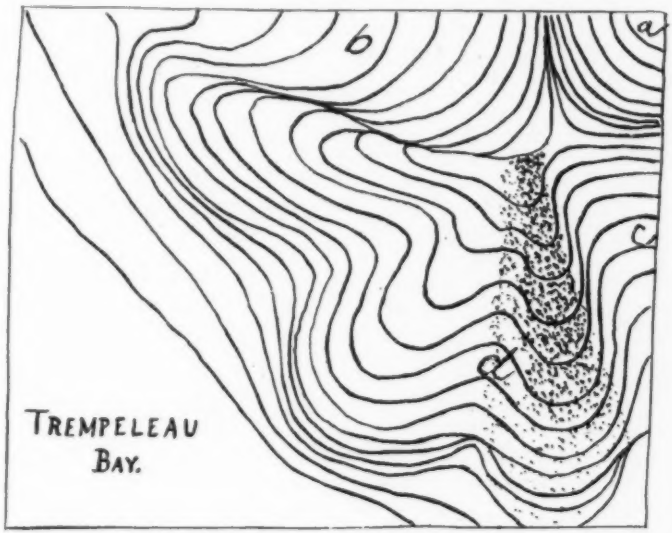


FIG. 6. Contour sketch map of a single buttress with a portion of its primary bluff, *a*, to illustrate the distribution of the limestone débris as shown in connection with the circs. Parts of two circs are shown, *b* and *c*.

The dotted portion shows where the limestone débris occurs. I have endeavored to show its relative abundance by the concentration of the dots. It should be observed, however, that while on the side furthest from the circ it is really represented by only scattered fragments, in the thickest part next the circ it is piled up to a thickness apparently of several feet. The side of the buttress toward the circ *c* is steep but not precipitous, but that toward *b* is a vertical escarpment several hundred feet long, and twenty to fifty feet high.

Scale 400′ to an inch. Contour lines at intervals of 25′.

to be more easily distinguished by the eye, the base of the limestone is represented by heavier contour lines). The dotted area on the east side of the buttress shows the portion covered by transported limestone. It is most abundant immediately adjoining a circ, diminishing as we recede from that, but not entirely

ceasing until we have passed the bottom of the nearest ravine (*d*, Fig. 6). When it occurs on both sides of a buttress, and there is but a single intermediate ravine, it extends in some degree over all parts. The maximum thickness of these accumulations is nowhere shown in section. Such indications as I have noted lead me to the belief that it will probably not exceed six feet or seven feet, thinning off until it is represented by scattered bowlders only. There is often an appearance as though the material had been thrown into subordinate ridges of low relief. They are too faint, however, to be relied upon as evidence, unless their reality can be confirmed by sections. The fragments range from massive or tabular forms several feet across down through all grades, and they lie as close together as the fragments in a macadamized road. The slopes from the circs to the bounding buttresses are nearly always steep (35° to 40°). That overlooking the circ *b* (Fig. 6) is vertical for heights varying from twenty to fifty feet. The small valley shown at *a*, Fig. 4 has somewhat the character of a circ. The sharp ridge which forms its western rim, or buttress, has a train of limestone débris for the greater part of its length, sometimes rather straggling, but quite abundant on its knoblike outer end, and the terminal slope, at the bottom of which it connects with the ridge shown in Sec. 5, Fig. 2 (previous article).

The larger valleys.—Of the occurrences in the larger valleys two examples are here sketched. The first, Fig. 5, is found in the largest of the valleys (the third described in previous article). The highest portion of its rim lies toward the northeast (compare Fig. 4), and a portion of this, a peak of triangular form, is shown in the figure. The northwest-southwest portion is a part of the valley rim, while the spur, *a*, projects into the valley. The top of this spur is thickly covered with limestone débris, save the inner end, and the slope of the main bluff up to the base of the limestone, where it is lacking. Whether the deposit on the spur is continued eastward and southward into the valley cannot be told on account of the loess. The second example, shown in Fig. 7, occurs in the easternmost of the two

confluent valleys first described, in which the highest portion of the rim is toward the northwest, and consists of a high and rather long bluff, somewhat crescentic in form. The north rim of the valley consists of a much lower ridge, nowhere reaching

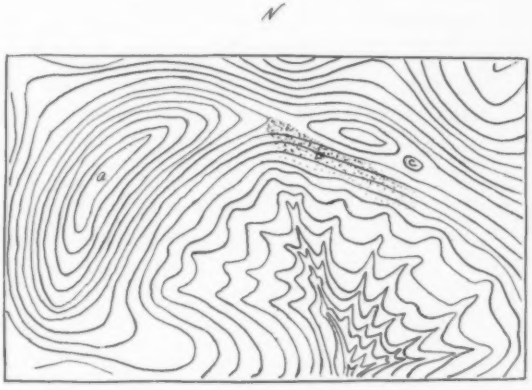


FIG. 7. Contour sketch map of the easternmost of the two confluent valleys first described in the last article. (The lower part is shown in Fig. 1.) The enclosing rim of the valley reaches the limestone horizon at only two points. One of these is the long bluff, *a*. The other would fall a couple of inches outside of the lower right-hand corner and is the same as the one shown at *g*, Fig. 4. At *b* occurs the train of limestone débris. Its downward extension is concealed by loess. *c*, knob of hard, ferruginous sandstone.

Scale 800′ to an inch. Contour lines at intervals of 25′.

to the horizon of the limestone. In proceeding eastward from the base of the limestone at the northeast end of the high bluff, limestone débris is lacking for some three or four hundred feet. It then appears suddenly on the top of the ridge, extending to an unascertained distance downward on the inside. A little further eastward the upper edge of the débris begins to fall a little short of the top of the ridge (on the inner side), and from that point it continues to decline at a nearly uniform rate, making an angle of about 4° or 5° with the horizon. The limestone is in sufficient abundance to wholly cover the ground up to almost the extreme limit of its occurrence, when within the space of a foot

or two it fails entirely. The figure does not exaggerate the nearly straight course followed by the upper margin of the deposit (to avoid misapprehension I may say that throughout the article, in speaking of limestone, only that derived from the Lower Magnesian horizon forming the caps of the higher bluffs is regarded, the limestone found at lower levels and belonging to the Potsdam series being carefully excluded).

I would call attention to the fact that all the examples given lie over against projecting angles of the primary bluffs. To reach the position occupied, material must have moved in defiance of the law which requires that it shall travel along the shortest available course from a higher to a lower level. We are obliged to account not for a few sporadic cases of such transportation merely, but for such an abundance as really amounts to a concentration. I therefore feel justified in saying that under existing conditions it is quite impossible that the material should have reached its present lodgment by rolling from the higher bluffs. In the case of the deposits on the buttresses, I have speculated as to the possibility of their having been earlier accumulations antedating the formation of the circs. But the hypothesis fails to explain all the facts even in these cases, and is of no assistance whatever in cases like those illustrated in Fig. 5 or Fig. 7.

The only tenable hypothesis remaining, as it seems to me, is that the valleys were filled with wind-drifted snow, which was piled up around the higher bluffs, so that only their limestone tops rose above. In such a case the limestone débris which reached the upper edge of the drifts would sooner or later work downward to a lodgment wherever the slope and other conditions were favorable. This involves the further assumption that the snow had been compacted to practically the consistency of glacier ice. So far as I have been able to bring it to a test, this hypothesis explains the various peculiarities remarkably well. All the localities showing limestone débris thus far discovered, are in places where, under the hypothesis, they would have been most likely to occur. Compare Fig. 5 and explanatory

notes. The minor features of distribution also harmonize well with it; for example, in Fig. 6, limestone is wanting on the north side of the buttress. But this side is remarkably high and the height is maintained for a long distance, and the upper surface of a drift having the average slope would have fallen much below the top of the perpendicular escarpment. Conversely there are other buttresses so small that a drift having the average slope would have buried them completely, and these also are destitute of the limestone.

It is evident that on this hypothesis the limestone accumulations furnish data from which we may calculate approximately some of the dimensions of the drifts. In some of the circs the slope may have been as high as 20° in places, but the average appears to have been nearer 15°. In the valleys it was much less, apparently ranging from 10° or 12° down to 4° or 5°. The greatest vertical thickness appears to have ranged between 200 and 250 feet in the valleys and between 80 and 120 feet in the circs

The hypothesis does not necessarily imply that the big drifts developed glacial motion, since the transit across their upper surfaces might have taken place though the drifts were themselves stationary. But if we may accept the existence of large bodies of snow in the valleys, as probable, the indices of glaciation shown in the lower portions of the same valleys gain greatly in importance.

The facts above given regarding the distribution of limestone on the bounding buttresses of circs, seems to render it desirable that some notice should be taken of their low level deposits. Unlike those connected with the larger valleys, these are almost wholly external, and have the form of alluvial cones. As seen in section, they display the concentric structure lines indicating the successive stages of their upbuilding. These are more pronounced on either side of the center where also the material is usually fine.

Along the center, or axis, bedding planes are often faint, or lacking, and much heavy material is included. As might be

expected, the coarse material is a fairly representative assemblage from the different horizons.

So far there is nothing to suggest glacial action. But two bowlders seen in the railroad cutting are noteworthy on account of their character, and certainly suggest some such agent. They are tabular forms six or seven feet across, and two to three feet thick, derived from the thin-bedded, impure limestones of the Potsdam series and are extremely fragile. One, indeed, is divided near the middle by what has every appearance of being an old joint about half an inch wide, and the sides still parallel. Their nearest point of origin was several hundred feet distant. How such masses could have traveled even a short distance without falling to pieces, it is hard to see, unless they were firmly embedded in some matrix.

From a variety of circumstancs, I have the impression that the circs have been well cleaned of rocky débris, and that such material is now accumulating at their upper ends. The indications are strong that little save the finer débris now passes out. For the present, however, I should not care to lay much stress on these impressions.

While these deposits must be regarded as essentially nonglacial, there does not appear to be anything inconsistent with the assumption that occasionally during periods of exceptional activity, glaciers may have advanced on to them for short periods.

The field is very far from having been exhaustively worked, and until evidence is more nearly complete I prefer to reserve final expression of opinion.

G. H. Squier.

FUCOIDS OR COPROLITES.

THE middle part of the Devonian section seen along the Mississippi River between Hampton in Illinois and Muscatine in Iowa consists of a shaly limestone, which is quite rich in fossils.[1] From this horizon I have for some time collected certain structures, which have a close resemblance to the fossils described by James Hall under the name *Spirophyton*,[2] and which occur in the rocks of the Hamilton period in New York. The fossils found here consist of flat cakes of calcareous material, from one to six millimeters in thickness and from five to thirty centimeters in width, intercalated among the layers of the rock, mostly lying parallel with these, and presenting an endless variety of forms (Figs. 1–8). The flat surfaces are bent in a succession of wave-like, crescentic, low and wide ridges, which become confluent and indistinct near the margin. Generally the widest ridges have a corresponding depression on the opposite side of the cake.

Much of Hall's description of *Spirophyton* is perfectly applicable to these fossils. Their substance is often "scarcely separable from the stony matrix," especially when the containing rock is unweathered. The wave-like ridges are "frequently not distinctly limited on the outer margin," which then appears to be continuous with a lamina in the rock (Fig. 8). In one instance there is a shallow groove following the edge of the cake on either side (Fig. 1) and resembling that seen in Hall's figure of *Spirophyton typum*.[3] The substance of the cake contains fragments of "small shells or fragments of shells." One of these is

[1] The part of the section to which is here referred is No. 4 in my paper, A Brief Description of the Section of Devonian Rocks, etc., published in the Journal of the Cin. Soc. of Nat. Hist., Vol. XIX, No. 3, pp. 93–95.

[2] Observations upon some Spiral-Growing Fucoidal Remains, etc., 16th Report on the State Cabinet of Nat. Hist., JAMES HALL, 1861–2, 76–83.

[3] HALL, loc. cit., Pl. II, Fig. 2.

apparently a minute pteropod. I have, however, been unable to find any spiral or helical forms like that of *Spirophyton typum*. The nearest approach to it is a twisting of that end of the cake toward which the concave sides of the ridges are directed (Fig 7).

There are several circumstances which point to a mechanical origin of these structures. The material of which they consist is a compact calcareous mass apparently identical to that of the surrounding rock. It is difficult to explain how this could have been introduced in such quantity and in such condition into the interior of a soft pulpy seaweed, and still have permitted the plant to leave a mold of both of its surfaces in the accumulating sediments. Another circumstance of similar significance is the indistinctness of the margins of some of the specimens. In their shapes, moreover, there is an indefinable lack of that uniformity of design which we are accustomed to find in organic forms. Unless certain ones are selected and others left out, classification on this basis seems impracticable. From Hall's description of the spiral-growing fucoids in the Devonian rocks in New York, it is evident that these also are variable in form, for he states that "the larger fronds not unfrequently present irregularities and distortions, both from unequal growth and from accident, evidently having been very flexible and easily disturbed," and he refers to some of the specimens as being "detached portions which have been distorted by pressure after their separation."

EXPLANATION OF PLATE VII.

FIG. 1. Dimensions: length, 19^{cm}; breadth 5.3^{cm}; average thickness, 2.5^{mm}.

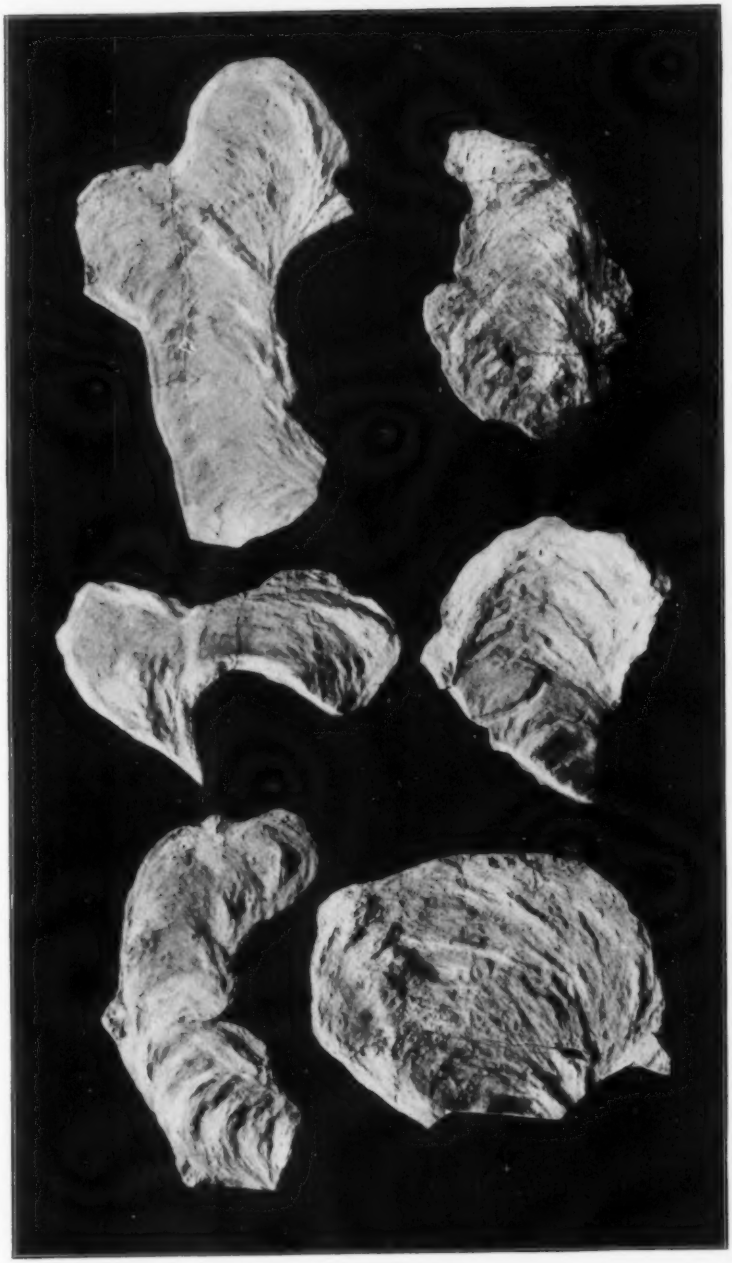
FIG. 2. Dimensions: length, 12.5^{cm}; breadth 8^{cm} at the widest point; average thickness, 2^{mm}.

FIG. 3. Dimensions: greatest diameter, 13^{cm}; average thickness, 4^{mm}. Evidently fragmentary.

FIG. 4. Dimensions: length, 12^{cm}; greatest breadth, 10.5^{cm}; thickness from 6^{mm} to 2^{mm}.

FIG. 5. Dimensions: length, 16^{cm}; average breadth, 5^{cm}; average thickness, 4.5^{mm}.

FIG. 6. Dimensions: greatest diameter, 14^{cm}; average thickness, 4.5^{mm}. Fragmentary.



The general appearance of the western specimens is such as to suggest that they have been formed from flowing mud. In some instances the crescentic ridges overlap, as if the flow had run over on itself. Near the edges of the specimens planes of divisions are sometimes seen, which readily might be accounted for as planes of differential motion in moving mud, but which seem difficult to explain if the specimens be regarded as imprints or casts of sea-weeds.

But the resemblance between these western specimens and *Spirophyton* is in general so perfect that there can hardly be any doubt that both have the same origin, and it is evident that the regular spiral frond of the latter, however rarely found, cannot have been produced except by the intervention of some organic agency. The quiet sea-bottom indicated by the nature of the sediments containing the fossils also preclude the possibility of the occurrence of mud-flow in the ordinary way under the conditions of deposition of the containing rock.

It seems, however, that cakes like these might be formed in just such situations from the voided contents of mud-eating animals, such as sea-cucumbers. These are known to burrow in the mud, far out in the sea, and to extract their food from such fragments of organic substance as they find in the ooze, mud, and sand with which they fill themselves, and which is afterward expelled by contractions of the visceral muscles. This may, no doubt, take place down in the mud as well as on the surface of the sea-bottom. If we suppose that the cakes have been formed in such a way, it is easy to account for their diversity of form—even when this takes on the complex twist of a spiral—and there will be no difficulty in explaining how it came about that they are molded from the same material as the surrounding matrix. A loose mud slowly forced out from a receding tube

EXPLANATION OF PLATE VIII.

FIG. 7. A specimen in the matrix. Length, 12^{cm}; breadth, 10^{cm}; thickness, 2^{mm}.

FIG. 8. Fragment of a large specimen cut by a joint in the rock obliquely, and confluent at the edge with a layer in the rock. Greatest length, 21.5^{cm}; breadth, 14^{cm}; average thickness, 4.5^{mm}.

can be made to settle in heaps which will have a noticeable resemblance to these peculiar fossils. This new interpretation of an obscure class of objects is given merely as an aid in their study. Though the holothurians are referred to as the animals most likely to produce coprolites of this kind, it is believed that any other mud-eating species of burrowing habits may just as well have done the work — soft animals, perhaps, that were sufficiently protected by their concealment in the ocean mud to render unnecessary any hard parts, which might have left less uncertain traces of their existence.

J. A. UDDEN.

ROCK ISLAND, ILL.

ZIRKELITE—A QUESTION OF PRIORITY.

In the *Mineralogical Magazine*, Vol. XI, pp. 86–88 (read June 18, 1895) is described a new mineral containing zirconium, titanium, lime, iron, etc., under the name of Zirkelite. This paper was prepared by my friend, Dr. E. Hussak, and by Mr. C. T. Prior. Later Mr. Prior (*loc. cit.* pp. 180–183, read Nov. 17, 1896) published an analysis of the same mineral.

I wish to protest against the use of the name Zirkelite for this mineral on the ground of the prior use of it to designate a commonly occurring rock belonging to the basaltic family.

When two subjects are so intimately connected as mineralogy and petrography it does not seem to be for the interest of science that names should be duplicated in them. So true is this that I abandoned the name Rosenbuschite, which I had given to a class of rocks in honor of Professor Rosenbusch, because only a few weeks previously it had been employed to designate a new mineral.

The term Zirkelite was used by me in 1887, or seven years before it was taken by Messrs. Hussak and Prior. (See Preliminary Description of the Peridotites, Gabbros, Diabases, and Andesites of Minnesota, *Bulletin No. 2*, Geological Survey of Minnesota, 1887, pp. 30–32.) It was used to designate the commonly occurring altered conditions of basaltic glassy lavas which are often called diabase glass, etc. Zirkelite occurs forming the entire mass of thin dikes, and the exterior parts of many dikes of diabase and melaphyre, as well as the surface of old lava flows like the melaphyres and diabases of Lake Superior, Newfoundland, and elsewhere. Zirkelite holds the same relation to tachylite that diabase and melaphyre do to basalt, *i. e.*, an older and altered type. The macroscopic and microscopic characters of this rock were given in the locality cited above.

The term Zirkelite was again used in the same way in my *Report of the Geological Survey of Michigan* for 1891–2 (1893, pp. 90, 97, 138, etc.).

It was also published in my classification of rocks given in the *Catalogue of the Michigan College of Mines* (Michigan Mining School), 1891–2, p. 104; 1892–1894, Table XI; 1894–1896, Table XI.

Further the term Zirkelite is defined in accordance with my usage in Lewinson-Lessing's *Petrographisches Lexikon*, 1893, p. 252); and accounts of it are given in the *Neues Jahrbuch für Mineralogie*, 1893, II, p. 292, and in Kemp's *Handbook of Rocks*, 1896, p. 170.

M. E. WADSWORTH.

MICHIGAN COLLEGE OF MINES, HOUGHTON, MICH.,
December 17, 1897.

[The prior use of the name *Zirkelite* is certainly established, but it is a question how far a petrographer is justified in stigmatizing the name of a fellow worker by attaching it to an indefinitely decomposed and ill-defined rock.—J. P. I.]

EDITORIAL.

Editors Journal of Geology:

I WOULD like to call attention to the constant misuse of a foreign word much used by glacial geologists. The word *aas* in the Danish, *ås* in Swedish, which in Norway is used for a rounded hill, and in Sweden and Finland especially for those long gravel ridges which are clearly of glacial origin, has its plural *åsar*. The word is pronounced like the *oas* of *boast*, and really the best way to transliterate it into English would be to spell it *oas* rather than *os*, as the long sound would then be more certainly given it. The plural would then be *osar* or *oasar*, which is, perhaps, preferable to *oases* for obvious reasons.

Therefore the writers who speak about "*an osar*," and mention "*the osars*," are producing the same kind of horrible hybrids that foreigners would who should speak of "*an oxen*" or "*I saw three mices*," mistakes of a kind which, in writing, are unnecessary.

L. V. PIRSSON.

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ANY protest that will help to a better use of terms in science or elsewhere is to be welcomed as a contribution toward the relief of one of the most grievous burdens of the intellectual world. The incompetencies and inadaptabilities of our vehicle of thought, to say nothing of its absurdities, are already most serious obstacles to intellectual progress, and they are daily growing in intensity and threaten to become altogether unendurable in the near future. In former times, when the substance of thought was limited, the intellectual gymnastics involved in mastering the idiosyncrasies of language were not without their compensations. But the time has come when even the essence

of the most imperative thought has grown to such magnitude that any labor wasted upon the trammelings of verbiage falls into the category of the reprehensible. It would be an interesting investigation which should show how great an amount of ignorance of vital truths is justly chargeable to the time consumed in gaining a questionable mastery of the needless, not to say the positively pernicious, factors of a language whose evolution is a century behind the times. It would be an instructive investigation in criminology which should ascertain how much of suffering, death, and other disasters arise from crowding aside instruction in vital matters to make room for the dull grind upon the senseless conventionalities of a delinquent language.

To the already deplorable state of things chargeable to linguists, teachers, and the common public, the devotees of science are adding their special inflictions, and if present practices continue there will apparently be no remedy in the future but open rebellion. It is said that the number of organic species and varieties has grown already into the neighborhood of one million, and each of these is burdened with a binomial, if not a trinomial, designation consisting usually of an artificial breccia of Greek, Latin, local, personal, and other verbal fragments, rudely stuck together and finished off at the end in Latin fashion. They are "neither fish nor flesh, nor good red herring."

Our mineralogical terminology which endeavors to impose on all futurity unmouthable distortions of the names of insignificant streams or mountains or villages or collectors or scientific friends unworthily rivals the biological monstrosities. And when we come to compound these into the names of rocks, in pursuance of a most natural and laudable system of nomenclature, their uncouthness is more than doubly emphasized, and becomes almost prohibitory. If geologists in their own field are not coequal sinners, it is, perhaps, only due to a less urgent need for terms. When we contemplate that to which this inconsiderate practice will inevitably lead as the number of varieties and species and distinctions increase with the progress of research, the seriousness of the evil becomes intensified. When

we compute the loss to the acquisition of the essential elements of science which must ensue if this system is perpetuated through the five to ten million years which the solar prophets assign as the possible future of the habitable earth the magnitude of the affliction grows to prodigious dimensions. It may be confidently predicted, however, that there will be a revolt in the not distant future if a rational movement toward reform is not soon inaugurated. In the meantime every little reform has an importance, not only in its own merit, but also in its moral effects as a step towards general reform.

Back of the special criticism of Professor Pirsson there is a general question which invites attention in connection with his protest: What considerations shall guide us in the endeavor to secure better practice? The word *ås, åsar*, was anglicized to osar, osars, a half century ago, and may be found current in the writings of Murchison, Desor, the elder Hitchcock, and others. Practice has been divided ever since between the alternative evils of introducing into the English language a word of irregular variation and uncertain pronunciation (to English people) with its consequent infelicities, or of ruthlessly modifying the Swedish word to suit English practice, with the barbarisms which Professor Pirsson points out. There is, however, a *tertium quid*, to which Americans are quite generally turning. It is the avoidance of both these alternatives and the adoption of the term *esker* instead, which in form and phonic nature is more acceptable. Whatever may be the method of formation of the plural of the ultimately perfected world language, it is quite certain that it will not follow the Swedish analogy *ås, åsar*, because this is not inherently meritorious. Hence we do the English language a poor service and put obstacles in the way of the ultimate common language by introducing a form not in itself worthy to endure. It may be urged as an objection to the term esker that *ås* had currency at an earlier date. If we are to give the law of priority its widest application, and bow unhesitatingly before it, the objection holds good. The writer has himself previously yielded to it. But on fuller consideration he withdraws from this position and favors

the use of the inherently preferable term esker. While due regard should doubtless be paid to the law of priority, it seems obvious upon mature consideration that all future generations should not be made to suffer unduly for the infelicities of the first usage often carelessly inaugurated. The improvement of the language should have first thought, and be given determinative weight in all permissible cases. The evolution of a common vehicle of thought for all the world will grow more imperative as intercommunication and common sympathy become more universal, and the rapid increase of vital knowledge and the more strenuous demands of a higher civilization will require that this vehicle shall be not only surpassingly rich in its resources, but economical in its modes of operation. Intellectual wastefulness is as reprehensible as material wastefulness, and our vehicle of thought should be as assiduously improved in the interest of economy and effectiveness as our vehicles of property or person.

T. C. C.

Reviews.

Fourteenth Annual Report of the New York State Geologist for 1894. JAMES HALL, State Geologist.

This volume of 669 pages, besides containing the short report of the state geologist giving an account of the work done under his direction, embraces several valuable papers upon the geology and palæontology of New York state. The brief reviews of these papers here given are taken largely from those written by the state geologist and printed in his report at the beginning of the volume.

1. *A Preliminary Description of the Faulted Region of Herkimer, Fulton, Montgomery, and Saratoga Counties.* By N. H. DARTON, pp. 31–56, Pls. I–IX, Figs. 1–12.

The field work upon which this paper is based was done in connection with the preparation of the geological map of the state. It describes in detail a region of country which was originally described by Vanuxem in his report on the geology of the Third District. It gives an account of the general relations of the faults, and describes in detail those at Little Falls, on the East Canada Creek, St. Johnsville, the Noses, Fonda, Tribes Hill, Broadalbin, Hoffman's Ferry, Saratoga, and Lake George.

The region is a general monocline with sediments of slightly varying dip, and the faults traversing this monocline, accompanied by certain features of local disturbance, have considerably modified its regularity. As a rule, these displacements do not make conspicuous features in the topography, but one of them, at Little Falls, gives rise to one of the most striking features in the scenery of the Mohawk valley.

2. *Report on the Structural and Economic Geology of Seneca County.* By D. F. LINCOLN, M.D., pp. 57–125, Pls. I–XIX, Figs. 1–30.

The subject treated in this paper is covered under three general divisions: (1) surface geology; (2) stratigraphic geology; (3) eco-

nomic geology. Under the first head are given detailed accounts of the topography, and also of the superficial accumulations, their nature, and distribution. The sections on stratigraphic geology considers each formation in succession, from the Salina to the Portage, giving the local development and variations of each with fullness and precision. Faunal characters are touched upon to some extent, no wide difference in these respects from adjoining regions being noted. Under the head of economic geology are considered all the rock products of the county, their mode of exploitation, treatment, and economic value.

3. *The Principles of Palæontology.* By FELIX BERNARD. Translated by C. E. BROOKS, pp. 130–215.

This paper is extracted from Bernard's *Eléments de Paleontologie*, Paris, 1895, and is translated and here published for the benefit of American students to whom Bernard's entire works may not be accessible. No other writer has succeeded in setting forth so clearly the actual condition of the science, its relations to other departments of knowledge, and the inherent importance of the problems with which it is wholly concerned.

4. *Development and Mode of Growth of Diplograptus, McCoy.* By R. RUEDEMANN, pp. 217–258, Pls. I–V.

The observations recorded in this paper are based upon material in a remarkably perfect condition of preservation, obtained from the Utica slate at Dolgeville, N. Y. The paper shows that these graptolites, generally occurring as isolated stipes, were actually colonies composed of a large number of such individual stipes, growing radially from a center. The structure of the central part of the colony is shown to consist in (1) a central floating sack or pneumatocyst, demonstrating that the colony was unattached; (2) a verticil of spherical gonangia, within which are found masses of young graptolites or siculæ attached to a central axis; beneath the gonangia are (3) the radiately arranged graptolite stipes attached by long, bare extensions of the axial rod or virgula of each stipe. The paper is illustrated with five plates of highly instructive drawings.

5. *A Revision of the Sponges and Cœlenterates of the Lower Helderberg Group of New York.* By G. H. GIRTY, pp. 259–322, Pls. I–VII.

In this paper the known species of the groups mentioned are redescribed, with one new genus and ten new species. Four genera of

sponges, with five species, are recorded, with thirteen genera and twenty-one species of cœlenterata.

6. *New Species of Brachiopoda described in the Palæontology of New York*, Vol. VIII, Parts I and II, 1872–1892. By JAMES HALL, pp. 323–402, Pls. I–XIV.

In this paper are published the descriptions of 106 species of brachiopoda, which were described incidentally and sometimes figured without descriptions in the recent work upon the class by Hall and Clarke.

7. *A Handbook of the Genera of North American Palæozoic Bryozoa.* With an Introduction upon the Structure of Living Species. By G. B. SIMPSON, pp. 403–699. Pls. A–E, I–XXV, and 222 figures in the text.

The first portion of this work is devoted to the recent bryozoa, and contains the history of observations upon these organisms from 1599 to the present time, followed by a bibliography and an illustrated detailed account of the anatomy.

The second part is devoted to the fossil forms from the Palæozoic rocks, and contains a scheme of classification, the bibliography of the Palæozoic species of America, a list of the genera and species described, with references to authorship and the geologic formations in which they occur. The genera described number 156, the species enumerated are about 1100. The main portion of the second part is devoted to diagnoses of the genera, illustrated by 222 figures in the text and by 25 plates. STUART WELLER.

Petrology for Students, An Introduction to the Study of Rocks under the Microscope. By ALFRED HARKER, M.A., F.G.S. Second Edition, Revised. Cambridge, England, 1897.

A review of the first edition of this book by the present writer appeared in this JOURNAL, Vol. III, 1895, p. 856. The present edition reproduces the original text, with slight alterations, some of which follow the changes that appeared in the third edition of the second volume of Rosenbusch's *Mikroskopische Physiographie*, etc., published in 1896; besides the addition of numerous notices of American and Norwegian occurrences of various rocks, with references to their descriptions. The fuller mention of American occurrences increases

greatly the value of the book for students in America, who will find it very useful for this reason.

The alteration in the title of the second group of igneous rocks, according to the classification followed in this book, namely, from that of Intrusive to that of Hypabyssal, has not obviated the necessity for the apology made in the introduction to this group of rocks in the first edition, which is repeated in the second. The newer term is as inappropriate as the former one, and the criticism made in the review of the first edition holds with equal force in the present case.

J. P. I.

Rocks, Rock Weathering and Soils. By G. P. MERRILL. 8vo. 411 pp., Macmillan & Company, New York, 1897.

This admirable work brings together three subjects closely consecutive in the processes of nature but not previously associated as the subject of equally elaborate treatment in their mutual relations. The main emphasis of the work is placed on rock weathering, the description of rocks being in the main preliminary to this and that of soils a natural sequence. No attempt is made to treat rocks as such in an exhaustive way, nor soils as such. The discussion of weathering on the other hand is made as exhaustive as the present state of science will permit. The 168 pages of Parts 1 and 2 relating to minerals and rocks embrace a reasonably satisfactory treatment of these themes. This is as much perhaps as can be said of any attempt in this line in the present unfortunate condition of the classification and nomenclature of rocks and minerals. The relative fullness of treatment of the several rocks is measured in a degree by their importance in the production of soils. Very properly prominence is given to chemical composition, since this is a prime consideration in following the transition of the rocks into soils and secondary rocks. The numerous tables of analyses are a valuable feature. The use of terms is conservative and many of the intermediate stages in the gradation of one rock into another are left without specific nomenclature. The author files a protest against the tendency "which has resulted already in such monstrosities of nomenclature as *ouachitite*, *monchiquite*, *yogosite* and *absarokite*."

The subject of weathering and transportation occupies the heart of the book and constitutes its distinguishing feature. After a statement of the principles of weathering and of the agencies involved, the special

modes of alteration of the leading rocks are discussed in detail. Perhaps the most valuable contribution of the book is the series of analyses of identical rock at varying stages of decomposition, by means of which the nature of the process, in so far as it is chemical, is specifically and precisely indicated. These tables show in just what degree the process acts differentially upon the several constituents of the rock. Although the analyses are not sufficiently numerous to warrant very broad generalizations, they are very helpful in giving approximate knowledge of the relative parts played by the several constituents of rock in the disintegrating process. The results of the analyses are conveniently indicated in separate columns which severally show the percentage of loss for the entire rock, the percentage of each constituent saved, and the percentage of each constituent lost. These special studies are followed by a résumé embracing general deductions drawn from them.

The chapter on the physical manifestations of weathering treats of the more familiar effects of the process on texture, color, surface configuration and similar features. This is followed by an interesting chapter on time considerations, in which are treated the rates of weathering and the influence of position, texture, composition, humidity, temperature and other climatic conditions upon the progress of the process.

The mantle of loose material which results from the weathering, together with loose material accumulated on the surface by other agencies, the author designates *regolith* (mantle rock), and devotes the last 100 pages to its description. It is not altogether clear whether the simple fact of mantling the surface with loose material is sufficient to unify accumulations arising from quite diverse agencies and varying greatly in nature, and hence to call for a specific name of the petrographic form. The residuary clays and earths constitute a unitary formation derived directly by the processes of weathering. The glacial, eolian, and similar deposits can only be brought into the same category by largely neglecting their mode of origin and confining attention merely to their superficial disposal and their incoherent character. It may well be questioned whether the genetic factor in these cases will not usually be the one to be kept at the front, and be more often placed in contrast to the residuary earths than merged with them. Doubtless, however, the mantling feature which they possess in common will make the term regolith often convenient. The word at any rate may be left to stand or fall as experience shall dictate.

The discussion of the soils is relatively less satisfactory than most other portions of the book, but this is a subject so large in itself that a satisfactory treatment could not be expected as a theme subordinate to so broad a subject as the central topic of the book.

The essence of several of the sections on weathering were published in this JOURNAL while the author was engaged upon the studies which have taken form in this book and its readers are familiar with the excellent method of their treatment and their substantial character.

C.

Authors' Abstracts.

PAPERS READ AT THE MONTREAL MEETING OF THE GEOLOGICAL SOCIETY OF AMERICA.[1]

Topography and Glacial Deposits of the Mohawk Valley. By Albert Perry Brigham.

The lower Mohawk, and a corresponding valley to the westward, are considered as subsequent in character, having been initiated by headward cutting from the ancient Hudson and St. Lawrence valleys, along the strike of soft beds, to the col located by Chamberlin at Little Falls. The Adirondack streams consequent on Palæozoic topography were thus diverted and the Susquehanna streams were beheaded. West of Little Falls the rock floor descends toward Lake Ontario, but not uniformly, a buried rock basin above 100 feet in depth, lying east of Utica. The present arrangement is due to glacial and aqueo-glacial erosion at Little Falls, and to aggrading from Rome eastward by glacial materials. The westward flow of the lower Mohawk glacier is confirmed by striation at Amsterdam.

The drift deposits west of Little Falls are largely composed of deltas and benches whose altitudes indicate approximately a water level of 600 feet. This is believed to represent a lacustrine stage in which the waters had fallen below the Warren level and below Fairchild's Geneva beach, but had not yet subsided to the Iroquois plane. The dam is thought to have been at Little Falls, and of a composite nature — the sill of gneiss then standing at about 440 feet, with drift and ice blockade, in this long, sinuous, narrow gorge. Below Little Falls, marginal bodies of massive till, aggraded by water-laid material, show a fluvio-lacustrine level of 430 to 440 feet, the barrier being unknown. The next stage in the lower valley was also fluvio-lacustrine, at 340 feet. The gneiss then caused a great waterfall at Little Falls, and the lacustrine stage persisted to the eastward, while a rock gorge more than

[1] Continued from last issue.

100 feet deep was cut at Aqueduct, near Schenectady. Certain beds of massive, water-laid clay west of Little Falls, taken with similar deposits in the Chenango and Unadilla valleys, are thought to show long and quiet deposition, with perhaps considerable later erosion, before the last advance of the ice across central and southern New York.

Clastic Huronian Rocks of Western Ontario and the Relations Between Laurentian and Huronian. By A. P. COLEMAN.

The rocks of the Lake of the Woods and Rainy Lake regions of western Ontario have been excellently mapped by A. C. Lawson, who calls them Archæan and subdivides them into a lower part, the Laurentian, and an upper one, the Ontarian, further subdivided into the Couchiching and the Keewatin. The Laurentian, which consists chiefly of granite and gneiss, underlies the other two series, but has an eruptive contact with them, showing that it was the latest in age. The Couchiching is formed mainly of fine-grained gray gneiss and mica schist, of clastic origin, since the quartz is usually in distinctly rounded grains. These rocks merge into almost unchanged sandstones in a few places, as found by the writer.

The Keewatin is much more varied, consisting very largely of basic and acid eruptives with their pyroclastics; but containing also important sedimentary members, such as limestone, slate, quartzite and conglomerate. The last rock is not a basal conglomerate resting on the Couchiching or Laurentian, but comes high up in the series, since it contains mainly pebbles of eruptives and schists found in adjoining portions of the Keewatin. It may represent a break equivalent to that between the lower and upper Huronian in the states to the south, as described by Van Hise. These conglomerates contain no Laurentian pebbles so far as known.

The field relations of the three formations are very interesting, both as mapped by Lawson and as observed on the bare shores of lakes in the western Archæan peneplain. The Keewatin, and in the southern part of the region the underlying Couchiching, form sharp synclines, curving as wide meshes round the areas of Laurentian, which vary greatly in size, running from a diameter of less than a mile to about fifty miles. Starting from the center of a Laurentian area one commonly finds first granite, then gneiss having a strike parallel to that of the adjoining schist. Before reaching the schist many frag-

ments of it are usually seen embedded in the gneiss. Green Keewatin schists are generally turned to hornblende-schist near the contact, but the Couchiching mica-schists are seldom much altered. Dikes of granite or felsite frequently run from the Laurentian gneiss into the Ontarian or Huronian rocks, as they are generally called by Canadian geologists. We have evidently here a section through a pre-Cambrian mountain group, so near its base that some of the meshes run out as unfinished curves, erosion having eaten completely through them. From the steep dip of the schists in the synclines and the width of some of the Laurentian batholites we must infer that these dome-shaped mountains were of considerable height, probably comparable to the highest present ranges.

Lawson has computed the thickness of the two members of the Huronian (Ontarian) at about five miles each; so that in some places 50,000 feet of sediments, in the upper part mixed with eruptives, must have rested on the old sea floor; as great a thickness as we find in the sediments preparing the way for later mountain ranges.

This brought about a rise of the isogeotherms sufficient to produce hydrothermal fusion of the rocks underlying the sediments.

As the usual theory of mountain building, by lateral thrust, can produce only folds, these domed mountains must have been elevated in some other way. They may be compared with Gilbert's laccolites or I. C. Russell's plutonic plugs, or perhaps more nearly with the structure of the Black Hills, but present important differences from all of these types of mountains.

The writer suggests that the hydrothermally fused acid Laurentian magma was lighter, both because of its heat and specifically, than the overlying rocks; and so, by the laws of hydrostatics, slowly crept toward the points where the load was smallest, the heavier Huronian rocks sinking toward the lower portions, where they were ultimately nipped in as sharp synclines.

The region which typically displays this system of Huronian meshes enclosing Laurentian batholites is more than two hundred miles long and a hundred and twenty broad. How much farther similar conditions prevail cannot be known until the Canadian Archæan is more completely mapped than at present.

The Laurentian has been shown to form eruptive contacts with the Huronian eighty miles north of the Lake of the Woods, and in the Sudbury district, five hundred miles to the east. The Hastings series,

as well as the Grenville series in eastern Ontario and Quebec, probably the equivalents in age of the western Huronian, show similar curving bands and eruptive contacts of the underlying Laurentian, as described by Adams, Barlow, and Ells; and the same is true, in part at least, of Labrador, as described by Lowe.

On the other hand, the Huronian regions in the United States south of Lake Superior, and also in New Brunswick, present a basal conglomerate resting unconformably on the Laurentian, according to Van Hise and Dawson. It may be that in the latter cases the thickness of sediments was not great enough to depress the Laurentian floor to the level of fusion or plasticity; or that the Huronian, as recognized in these regions, is really younger and overlies the upturned edges of the rocks described as Huronian in the northern Archæan. Some remarks in Van Hise's pre-Cambrian geology seem to suggest this.

It is likely that eruptive contacts of batholitic masses with overlying rocks exist under every great mountain chain; though the "Fundamental Complex" thus arising is disclosed only in the more ancient and therefore more deeply eroded mountain systems. Something like this has been shown to exist in British Columbia by Dawson, but of Jurassic age. Under the later mountain systems, however, the arching of anticlinal folds probably aided the uprising of the plastic base; and we may suppose that the core of granite and gneiss forms long belts rather than approximately round batholites.

The term Laurentian has been used in the paper to include granites and gneisses of later age than the Ontarian or Huronian rocks, following the custom of the Canadian geologists who have worked in western Ontario. As this use differs from Logan's original definition, it might be better to substitute another name, unless it shall appear that the relation described above is universal in North America, and that the supposed Huronian found to rest uncomformably on the Laurentian is really of later age than the true Huronian.

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